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## CHANGE IN PLANS FOR 1953 ANNUAL MEETING

The 1953 APA meetings will be held September 4-9 in Cleveland, Ohio. The meetings were originally scheduled to occur on these dates at Michigan State College. Recent developments, however, led the Council of Representatives to vote a change in plans.

As the time approached for the making of detailed plans for the 1953 meetings it became apparent that it would be difficult to fit so large a convention as ours into the currently available facilities of Michigan State. It also became apparent that the Department of Continuing Education at Michigan State would have to charge a total of from \$10,000 to \$14,000, in addition to charges for rooms and meals, for handling the convention. This sum was to be collected by either charging each member a registration fee or by collecting the total amount from the APA. The magnitude of this fee, the possible inadequacy of facilities for eating and sleeping, and the probable difficulties involved in helping 4,000 psychologists adjust to an institutional atmosphere tailored primarily for undergraduate habitation, added up to serious doubts about the desirability of going to MSC even in spite of economic and other advantages.

The facts of the situation, and the facts about other available facilities, were presented by mail late in February to the Council of Representatives. The Council voted 59 to 5 against Michigan State and stated a decided preference for Cleveland over other possible meeting places. The change to Cleveland apparently was not conducive to great sadness among MSC authorities, though the members of the MSC psychology department are disappointed that they will not be our hosts.

Headquarters in Cleveland will be at the Hotel Cleveland. Most of the meetings will occur there and in the Cleveland College Building of Western Reserve University, just across the square. A few meetings will occur at other hotels. An adequate number of sleeping rooms, including many at the currently modest rate of \$3.00, will be available in downtown hotels. Western Reserve University will be the host institution and Erwin K. Taylor will be convention manager. Detailed information on Local Arrangements will be published in the May *American Psychologist*.

# FACT AND FANCY IN PERSONALITY RESEARCH<sup>1</sup>

DONALD W. MACKINNON

*University of California*

**I**N discussing fact and fancy in personality research, it is not my intention to distinguish what we have established as fact from what we might like to fancy we have achieved. Nor is it the difference between our accomplishments and our autisms in personality research. The magnitude of our discrepancy score is too obvious to need pointing out. Rather, my concern is to inquire into the roles of fact and fancy in scientific research in general and in personality research in particular. Please note that the phrase in the title is not "Fact *versus* Fancy" but "Fact and Fancy," for my thesis is not that we accept the one and reject the other, but that we give thought as to how best to use both for the maximal benefit of personality research. Other phrases, "Fact and Speculation" or "Fact and Theory," would have served equally well, perhaps even better. Certainly we fancy and speculate and theorize, but at what level and at what distance from facts as such?

A quotation serves well as a beginning.

**Enthusiasm for Theory?** Yes, Psychology can use much of it. However, we will produce but an empty formalism, if we forget that mathematization and formalization should be done only to the degree that the maturity of the material under investigation permits at a given time (13, p. 10).

There is greater need today to heed these words of caution and warning than in 1940 when they were first written by Kurt Lewin. Psychologists were then still rather restrained in their theorizing. But during the past decade, and especially in the area of personality, theorizing and model building have been pushed, if not to a point of no return, at least to a point of diminishing return.

My questioning the value of model building—at least the extent of its current development and many of the forms which it takes—is not to be construed as a defense of raw empiricism in science. On the contrary, what characterizes any science as

well as distinguishes it from other sciences is not the domain of phenomena which it studies but the conceptual language which it employs.

Science has been defined as "the recording, augmentation, and rational correlation of those elements of our experience which are actually or potentially common to all normal people" (3, p. 14).

Concerning the first of these steps, recording, little need be said. It constitutes the starting point of science, involving the activities of observing and describing phenomena, and recording in systematic fashion the observations and descriptions which have been made. This first step, when well executed, leads to the second—an augmentation of knowledge—for observations placed in orderly sequence or properly grouped, through the relationships thus established, acquire more meaning than they possess as isolated elements of experience.

Concerning the third step, the rational correlation of phenomena, more needs to be said. Such rational correlation of the elements of the scientist's experience constitutes what the layman thinks of as explanation. But, as Dingle (3) points out, the term "explanation" is an unfortunate one because of its false connotation of finality of knowledge. In science no knowledge is final, nor does "explaining" a datum remove all mystery from it. All that is involved in "explanation" is the establishment of a relation between the phenomenon which is "explained" and other phenomena about which something, but not everything, is known.

In discussing this point, Dingle has called to our attention the fact that ". . . when Franklin 'explained' lightning he simply showed that it was connected with electrical experiments in the laboratory. A relation was thus established between those experiments and the lightning flash, so that knowledge of one phenomenon was available for interpreting the other: the phenomena were *correlated* with one another. This correlation contained all the explanation that was given. What-

<sup>1</sup> Adapted from the address of the President of the Division of Personality and Social Psychology at the meeting of the American Psychological Association in Washington, D. C., September 2, 1952.

ever was mysterious about the electric spark remained mysterious about the lightning flash" (3, p. 16).

In correlating rationally the elements of his experience, the scientist ceases to be a raw empiricist, if ever he was one; for, in concentrating upon certain elements of his experience as objects of his scientific study to the exclusion of others, he is already guided by theory of a sort even if it be nothing more than an expression of interest which he puts to nature in the form of a question.

Though he starts with observed facts, and, in the end, always comes back to them as the bedrock of science, the scientist in developing his science moves away from facts. He does so in two ways—in developing abstractions and in framing hypotheses.

In abstracting, the scientist *subtracts* something from his immediate experiences, focusing his attention upon a single quality or characteristic of many similar, yet individually differing, observations. Having abstracted the common quality and given it a name, the scientist has framed one of the simpler, but far from unimportant, concepts of his science. Since these concepts, which are formed by the process of abstracting a single quality common to many phenomena, stand for aspects of phenomena which can be immediately experienced by way of the sense modalities, they may be designated *phenomenal* concepts.

In developing hypotheses, the scientist *adds* something to his experience of phenomena. Hypotheses, like abstractions, are created in order to relate in a rational fashion apparently diverse phenomena, not, however, as in forming abstractions, by detecting a common quality in the phenomena themselves, but by inventing a fictitious entity, process, or event in terms of which the phenomena can be meaningfully related. Unlike phenomenal concepts, hypothetical constructs are in the moment of their creation, figments of the scientist's imagination, and may, for this reason, be referred to as *fictional* concepts.

Unless I misread the writings of others, the distinction which I have sketched between phenomenal concepts and fictional concepts is the same as that made by Reichenbach (18) between *abstracta* and *illata*, and, at least in part, the distinction which MacCorquodale and Meehl (14) have proposed between *intervening variables* and *hypothetical constructs*.

I see no difference between what I have called phenomenal concepts and what MacCorquodale and Meehl have urged we accept as the meaning of intervening variables. They suggest, however, a more restricted meaning for hypothetical construct than seems to me either fruitful or necessary, and for that reason I should like to urge a more liberal interpretation of the meaning of fictional concepts or hypothetical constructs.

As I read the history of science, two kinds of hypotheses have been employed:

1. Those which hypothesize the existence of some entity, process, or event which, though not now observed or observable, is assumed to be of such a nature as to be potentially verifiable by direct observation.

2. Those which hypothesize the existence of some entity, process, or event which is not now observed or observable and is of such an assumed nature as to be beyond verification by direct observation.

The first type of hypothesis would be illustrated by the assumption of a particular kind of brain pathology to explain, or, as I would prefer to say, to correlate rationally, certain abnormalities of behavior. In the moment of hypothesizing the particular neuropathology, it is not demonstrated, though subsequent search may reveal its presence. When, in this manner, an hypothesized entity, process, or event is observed, and its relationship to the to-be-explained phenomenon is established, the hypothesis is replaced by a scientific law.

The second type of hypothesis which assumes the *existence* of some entity, process, or event beyond verification by a direct observation of them is composed of two subclasses. The first of these are hypotheses which postulate happenings in the past, e.g., an hypothesis that would explain the solar system. The events thus postulated cannot be observed because the only time when they could have been observed has passed. The same difficulty exists, of course, for many genetic hypotheses. But if the hypothesized event correlates observations as well as it would were it a phenomenal reality, that is all that the scientist asks. The second subclass of fictional concepts consists of those which postulate existences or processes whose unobservability is part of their essential nature. Concepts of this type abound in modern atomic theory: electron, neutron, posi-

tron, meson; in biology, the gene; in psychoanalysis, libido and superego.

I would argue that fictional concepts need possess no properties and are subject to no laws other than those which are necessary and sufficient to enable them to correlate observations rationally. We are at liberty to shape such hypotheses as we please—provided they “explain” phenomena. In shaping them we are free from the trammels of phenomenal reality. We may even endow them with qualities contradicting our own experience. Such hypotheses, as Dingle has expressed it, “are employed for rational correlation and are not admissible to experience: they must have rational properties, but not necessarily sensible ones” (3, p. 47).

With much of this, I take it, MacCorquodale and Meehl would agree. At least I interpret as agreement their statement that the formulation of hypothetical constructs “involves words not wholly reducible to the words in the empirical laws; the validity of the empirical laws is not a sufficient condition for the truth of the concept, inasmuch as it contains surplus meaning; and the quantitative form of the concept is not obtainable simply by grouping empirical terms and functions” (14, p. 107). But here the agreement ends.

A special requirement to be met by all hypothetical constructs is proposed by MacCorquodale and Meehl that “their actual existence should be compatible with general knowledge and particularly with whatever relevant knowledge exists at the next lower level in the explanatory hierarchy” (14, p. 107). Now this is not the extreme form of scientific reductionism, such as Pratt (16) espouses, which insists that the only explanatory concepts for psychology are those of the more basic biological sciences of physiology and neurology. But it is the next thing to it, in so far as it demands, if not identity, at least congruence between the hypothetical concepts of psychology and the “facts” of physiology and neurology.

Ideally one would wish for such congruence of the sciences, and we may expect gradually to approach it. But to reject, as MacCorquodale and Meehl's position would force us to, any hypothetical construct in psychology because it would appear that no neural process is likely to have the *combination* of formal properties required, is to deny us the use of concepts which may have great value in ordering the facts of personality. And

who can say with certainty that subsequent development of neurology will not eventually demonstrate neural processes which actually do possess the combination of formal properties required by some of our current concepts of personality?

MacCorquodale and Meehl remind us that the “pyramid of the sciences” will presumably catch up with us someday and warn that we should start getting our concepts in order for that time, requiring of them that they be something more than metaphors—that they be “true” in the sense of having “some probability of being in correspondence with the actual events underlying the behavior phenomena” (14, p. 105).

Krech (10, 11, 12) goes even further. For him it is not enough that hypothetical constructs be congruent with relevant neurological knowledge. He insists that they be assumed neurological events and processes. But he emphasizes, as MacCorquodale and Meehl do not, the inadequacy of present neurological knowledge and concepts. For this reason he creates his own brand of neurology with the central concept of “dynamic systems”; but the properties of these dynamic systems derive more directly from psychological than from neurological data. Now, I have no quarrel with Krech's neuromythology, as one critic has called it, because it is not faithful to known neurological facts. I would defend to the limit his shaping of his concepts in any language that he chooses, provided, however, that they suffice to correlate rationally the phenomena of behavior. My criticism of Krech's dynamic systems is based on other grounds: (a) that they do not serve adequately to explain or to correlate rationally the particular aspects of behavior which are central in the field of personality, and (b) that dynamic systems, for which Krech claims a greater reality because of their asserted neurological character, are indeed no more neurological than, for example, libido, and no less psychological either.

Certainly there can be no quarrel with the requirement that the set of symbols (whether words or numbers) which is developed to explain phenomena must employ a language different from the language in which the phenomena are described. The language of constructs is clearly different from the language of data. But the conclusion, drawn by many psychologists, that the explanatory concepts in psychology must be those of some more

basic science, e.g., physiology or neurology, is a gratuitous one.

Having argued, then, for the role of constructs, even the most fictional ones, in psychology; and having asserted that they need not be cast in the language of physiology or neurology—though they may be—the sole requirement being that they serve to correlate rationally the behavioral phenomena, I shall now express my opinion that in personality research our theorizing and building of models have outrun activities more intimately concerned with observation and data collecting. Our greatest need for the more adequate study of personality is systematic observation and systematization of the data we collect, and this, I submit, is something other than theorizing. When that need has been met, our task will not be finished. Rather, then, our need will be for an over-all and integrating theory.

In other areas of psychology the pressing need may even now be for integrating models. Perhaps the "facts" of learning are now sufficiently well established to require such a unifying theory, though even that appears questionable to me.

Whether personality theory will, in the long run, be something different from psychological theory is a problem which cannot be answered now. Of one thing I am certain—that the theory of such processes as perception or learning cannot take over for personality theory though it can contribute to it. Much recent work which has been called a perceptual approach to personality has been so centered upon perceptual processes as to lose practically all sight of personality. Perhaps eventually the distinctions between "learning theory" and "perception theory" and "personality theory," to mention but three, will disappear and in their place we shall have a single and integrated psychological theory. But whether such a goal will be reached will depend, in part, upon learning psychologists, and perception psychologists, and personality psychologists agreeing upon a single and common set of variables significant for and relevant to all of their interests. Certainly they are still far from such agreement and consequently most present-day attempts to build a common theory are rather unrealistic. Perhaps some of the present impatience to force a single theoretical model for all of psychology might be lessened if we could remember that the progress of physics was neither slight nor unimportant during the long period in which there

were only limited and separate theories for the fields of heat, electricity, mechanics, optics, and hydrostatics.

The preoccupation in recent years with personality theory and the mounting Babel of theoretical models should alert us to a need for more facts and for the establishment of more low-order correlations of clearly defined variables. The more we succeed in establishing intercorrelations within sets of experimental variables, in other words, the greater our success in discovering empirical laws, the less the need for theory. Skinner's (19) description of much theory as a refuge from data misplaces the emphasis. It would be fairer to say that theory is an artifact of ignorance. Spence has argued that "If under experimental condition,  $X$ , the response measure  $R$  is always the same (within the error of measurement) then we have no need of theory" (20, p. 51). Since such invariant relationships are so rarely observed in the functioning of personality, our need for theory would appear to be indeed great. And contrast the situation in the field of audition, as described by Stevens and Davis, with that which obtains in the field of personality. They write, "Theories flourish on a certain sparseness of facts and wither in the face of abundance. When all the relations are known, alternative theories are no longer possible, and, if a present inventory of the facts of audition leaves little room for theories of hearing—in the 19th century meaning of that phrase—the situation must be accounted a sign of progress" (21, p. x).

It is, then, not a question of trying to get along without theory in our pursuit of understanding of personality. Our very ignorance cries out for theory. The question is, "What kind of theory?" And I would now answer, "Low-order theory; theory which is built at first in large measure out of concepts that are close to data."

Marx (15) has argued that in the early phases of scientific endeavor there is especial need for hypothetical constructs to fill in the gaps in what is known and to suggest possible rational correlations of the observations which are made. Hypothetical constructs with surplus meaning, that is, existential meaning, can be tolerated early in the history of a science, but, according to this view, must eventually be replaced by constructs more closely and necessarily tied to the data, in other words, by intervening variables which mean nothing more than certain stated empirical operations.

Certainly the transformation of hypothetical constructs into intervening variables is one mark of a developing science, but it is inconceivable that any science will ever reach an end state in which intervening variables will have entirely replaced hypothetical constructs. There will always, in every science, be need for ever more general laws to subsume the ever increasing body of lower-order laws. The history of science is a succession of ever closer approximations to a complete rational correlation of all the elements of experience, but, so long as such a complete rational correlation is not achieved, there will continue to be need of ever better hypothetical constructs.

Marx (15) has stressed the movement from hypothetical constructs to intervening variables. I should like to emphasize that the scientist, entering a new field of endeavor (and the psychology of personality is still a very young field) need not jump immediately from his common-sense observations to hypothetical constructs and then work back from them to intervening variables. This may be the path of the occasional genius in science. But there is a slower, and not unrewarding, path from observation to intervening variable to hypothetical construct, along which a great band of scientists can work productively. And there is one very great advantage in proceeding in this manner. The intervening variables, established upon the basis of observation of what goes with what, and, being in themselves only statements of empirical operations, may point the way more clearly to the properties of the hypothetical constructs most needed at any moment in the history of a science than can imagination and intuition working without the knowledge which careful observations of data supply.

To take the work of Freud as illustrative of my thesis is not to imply that Freud met all the criteria of the complete scientist or that psychoanalysis is a well-established or highly formalized science. But whatever our opinion of psychoanalysis may be, the work of Freud has made more difference for the study of personality than has the work of any other single investigator, and a review of the course of his labors is instructive.

Freud's work as a student of personality falls rather naturally into three periods in each of which there was, of course, a blending of fact and fancy, but in different proportions.

The first period extends from the early 1890's,

when Freud began his collaboration with Breuer, until 1910. During this period Freud was the observer, noting forms of behavior and relationships among them which the cultural taboos of his time had kept more conventional and less intuitive observers from noting. This was, of course, no period of mere data collecting. Freud's insights and intuitions led him to look for relationships and derivative meanings which earlier, largely neurological, theorizing had not even suggested. But this was, for Freud, a period in which he took as his observational data what patients said and did and remained close to these facts in his attempts at theory to explain them.

The second period, the ten years between 1910 and 1920, may be described as the period of Freud's most brilliant theorizing. Look at the *Collected Papers*; this was the decade of Freud's theoretical classics: his papers on repression, narcissism, etc. If Freud was wandering farther from his facts in this period, he still never lost sight of them, and his theories were, considering the complexity of the data he had unearthed, still rather parsimonious.

The last period of Freud's work, from 1920 until his death in 1939, was a time of ever freer speculation which Freud himself labeled as a concern with metapsychology. This was the period in which the notions of a life and death instinct were adumbrated.

There were, of course, deviations in Freud's work from the line of development which I have sketched. But the deviations which did occur serve only to prove my point that what is lasting in his work are the facts which he was the first to note and the more parsimonious intervening variables which he developed in his attempts to correlate rationally the irrational behavior which he observed. The fictional concepts of *Totem and Taboo*, written in the first period, have no place in current personality theory; but Freud's attempts, in his last period, at a typology of character still merit our attention.

Those who have been wont to criticize Freud for his speculative tendencies should remind themselves of what Freud had to say about the dangers as well as the merits of speculation and the basis of science.

He wrote that "[There are certain ideas which cannot be worked out] except by combining facts with pure imagination many times in succession, and thereby departing far from observation. We

know that the final result becomes the more untrustworthy the oftener one does this in the course of building up a theory, but the precise degree of uncertainty is not ascertainable. One may thereby have made a brilliant discovery, or one may have gone ignominiously astray" (4, p. 77). ". . . these ideas are not the basis of the science upon which everything rests; that, on the contrary, is observation alone. They are not the foundation stone, but the coping of the whole structure, and they can be replaced and discarded without damaging it" (5, p. 34).

It may be, as Freud has assured his readers, that his *most* speculative ideas may be discarded without damaging the basic structure of psychoanalysis, but one may well wonder whether the progress of psychoanalysis as science would not have been speedier without them.

A great deal of the work in clinical psychology today has been criticized as of little scientific value because it has not been more explicitly guided by theory. No doubt there is much that clinical psychologists do with their patients which contributes little to an advancement of knowledge concerning the development, structure, and function of personality. But unlike many theoreticians who look upon such work as even detrimental to the building of a science of personality, I see it as the slow gathering of a body of data which is vitally needed at the present stage of personality research. It is at least a question for debate whether the amassing of many facts about personality relatively unguided and uninspired by brain theory is today more detrimental to the development of an understanding of personality than the building of theoretical models which pay little or no attention to fairly large bodies of data already relatively well established by repeated observation.

Theoreticians will doubtless question not only my conclusion that we should pay more attention to determining facts but also my charge that this aspect of the total process of the scientific investigation of personality is being neglected today.

The current devaluation in personality research of what I have called the first phases of science takes many forms.

In the 1930's it was considered appropriate to make explorations in personality, but today one is made to feel pedestrian unless in his investigations

he is testing so-called deductions from a theoretical model.

In 25 years of teaching, I have served on many faculty committees to advise graduate students on their research plans for the doctorate. In recent years I have been struck by the increasing insistence that a research plan, to be approved, must be designed to test predictions made from a clearly formulated set of hypotheses; and this requirement is made of the student of personality no less than of the student of learning or of the sensory processes. It apparently makes no difference that in these meetings, members of the faculty propose contradictory hypotheses and make conflicting predictions; the student is, nevertheless, held to the requirement of a theoretical justification of his projected research. In some fields and in respect of some problems, even in personality, such a demand is legitimate, but in other areas we would be more honest with ourselves and more faithful to the true meaning of theory if we admitted that what we proposed was an exploration.

Another indication of our underestimation of the significance of the fact-finding process is the common tendency to treat the laborious task of collecting data as a chore to be given, whenever possible, to someone else. The notion that anyone can collect data, but that only a few can think creatively about them, and that the two processes can always and everywhere be carried out entirely apart from one another is widespread. Collecting data is regarded as a job for a technician, for the graduate research assistant, but not for the professor or high-powered researcher.

In the more venerable fields of science such a division of labor can often be made advantageously, but not so easily or so often in new fields and especially not in fields where the data are the interactions of individuals in complex situations and where the collector of data must, of necessity, be a participant observer, not only listening with a third ear but seeing with a third eye. The neglect would not be so serious if we would ourselves make the first observations or pretest the instruments of our invention before turning these tasks over to research assistants, but frequently even these preliminary interactions with our subjects are omitted.

Such disregard of the sources of our data is difficult to interpret except as the consequence of the social hierarchy in our science which grants greater prestige to activities the farther removed they are

from data. If, as we know, even the performance of rats in mazes is influenced by the way in which they are handled, on what grounds do we believe that human subjects, infinitely more sensitive and subtle, will be any less affected in their behavior by the way in which they are treated in interview, test, or experimental situation?

Another sign of our failure to pay proper respect to our data is our frequent unwillingness to spend sufficient time examining them closely and studying concretely the relationships which obtain among them. One example will serve for the many forms this neglect may and does take. The requirement of determining, prior to computing correlations, whether or not data are related in a linear or curvilinear fashion is not an abstruse one. It is recognized by every graduate student in psychology. But to make the test takes time, and, if the number of variables is large, the temptation is great to assume linearity and to compute a matrix of  $r$ 's. So great is the impatience to move away from the concreteness of data to the determination, at a more abstract level, of relationships among them, that the meaningful relationships and the degree to which they exist may never be found. That many forms of behavior are related in a curvilinear rather than in a linear fashion is an observation that has been made by both clinicians and social psychologists. But it is one thing to make such qualitative observations and another thing to establish them in precise and quantitative form.

One may think of making scatter plots as playing with data, but it is a form of play which can pay off quite handsomely. The close inspection of the relationship between two sets of data is the only way to insure their proper classification without which they cannot contribute to an augmentation of our knowledge. To omit this step may mean the difference between obtaining an insignificant  $r$  or a significant  $\eta$ .

Finally, I wish to question the assumption widely made in personality research and in psychology in general that facts "established" in the test of a theory necessarily deserve more confidence than do facts "established" in more frankly empirical studies.

It is, of course, well known that, if a large number of tests be given to a group of subjects and the scores on each test correlated with those on every other test, by chance alone a certain number of the

correlations in the matrix would be significant. Not only the psychologist with a penchant for theory but the statistician, too, will warn that, in the absence of theory as to what should on rational grounds go with what, little reliance can be placed in correlations thus obtained.

It is common knowledge to test constructors and it should be no secret from any personality researcher that, if tests yielding statistically significant correlations with the criteria are administered a second time to another sample drawn from the original test population, a certain number of the correlations will almost certainly show considerable shrinkage or even wash out entirely in this first cross validation. On second and third cross validations still other correlations may fail to hold up.

But, let it be noted, if the investigator has had some good hunches guiding him in the construction and selection of his tests—Guilford's (5, 6, 7) work on creativity comes to mind—even though these hunches are not of such an order as to justify their being called theories, and, if the criterion measures have been reasonably adequate and stable, I assert that the empirical investigator's "findings," the correlations which continue to hold up in repeated cross validations, may deserve more confidence than the "findings" reported in researches much more elegant in experimental design and "established" in the test of theoretical predictions.

The notion that tests of significance of a difference need not be as stringent for data collected in an experimental checking of an hypothesis as for data collected in more empirical studies appears questionable unless at the same time certain stringent requirements with respect to the theory that is tested are also met. In psychology, and especially in the field of personality, much passes for theory that is only a hunch, or guess. To test such pseudo-theories or pseudo-models with, as is often the case, a very small number of subjects, and to test them only once, as is again so often done, and to apply to data thus obtained a less stringent test of significance because presumably a theory or logical derivation has been tested, is indeed capitalizing on chance.

Because these dangers are recognized, scientists have enunciated a principle of repetition of experimentation. But for the most part we give lip service only to this principle. We proclaim that science is the most democratic of procedures; only

that which can be tested and reproduced by all investigators is to be accepted as fact. But how often do experimental psychologists repeat the experiments of their colleagues? The infrequency with which they do so bespeaks, in a curious way, an underestimation of the importance of factual findings.

Too often the insistence that an experiment be repeated is heard only when deep-seated beliefs, often having nothing to do with science, are threatened. One thinks of Rhine's experiments on ESP (17). But even here the issue is prejudged by most psychologists; they demand that the experiments be repeated, but by someone other than themselves. They "know"—let it be noted, on theoretical grounds—that the results cannot be valid, so why waste time in attempting to prove them false. In passing, it may be observed that the attitude of many experimental psychologists is only slightly less prejudiced with respect to the reported findings of the psychoanalysts.

Once in a while an experiment, whose hypotheses are sufficiently within the stream of current psychological theorizing to be accepted, meets with more interest than resistance and is repeated. One thinks of Zeigarnik's (22) investigation of the recall of completed and unfinished tasks, and of the Bruner-Goodman (1) studies of the influence of need upon perception. In these cases, as in many others, where conscientious efforts have been made to repeat original investigations, we know how difficult it has been to confirm the reported findings. Whatever facts or laws seemed to be established in the original studies have proved to be, if not untenable, at least in need of radical qualification. But in such controversial areas of research and theory, we at least have some idea of what is not known, and consequently continuing research can give promise of some eventual ordering and establishing of fact.

It is, however, the vast silent areas of psychological research and theory which should cause us disquiet, for here the noncontroversial single-occasion findings are accepted without question or check.

Psychologists in the field of testing cannot lay claim to a long virtuous past in respect of confirming their findings. Perhaps it was because they were the worst offenders that they were the first to reform. In any case, in recent years, and especially since Cureton's paper on "Validity, Re-

liability and Baloney" (2), the necessity of cross validation has been clearly recognized. Cross validation prior to publication of findings is fortunately well on the way to becoming established practice in this field. Until repetition of experimental studies also becomes standard practice, there is no reason to place greater confidence in their reported "findings" than in the reported "facts" of more empirical studies.

In summary, I should like to emphasize again that my criticism is not of theory as such, but of the impatience to develop elaborate theoretical models of personality before laying the necessary ground work of observations and abstractions from them. I am concerned lest we move too rapidly in personality research from the practical, empirical, and intuitive, to the abstract, rigorous, and formal, with the risk, to which Lewin (13) called our attention, of building logical superhighways which turn out to be dead ends leading nowhere.

We need hunches and hypotheses concerning the significant phenomenal variables of personality and inferences from behavior to the underlying dynamics of the person. I propose that it is better in personality research today to settle for something less than full-blown theoretical models, namely hunches and working hypotheses. I would urge that we set lower goals and expend as much energy in pursuing them as is now spent, in my opinion, prematurely and fruitlessly in the pursuit of goals too fanciful for the present state of personality research.

But lest I am thought to be championing a raw empiricism I should like to remind you that *ledge*, the second element in the word *knowledge* means sport (9). Knowledge is the result of playing with what we know, that is, with our facts. A knowledgeable person in science is not, as we are often wont to think, merely one who has an accumulation of facts, but rather one who has the capacity to have sport with what he knows, giving creative rein to his fancy in changing his world of phenomenal appearances into a world of scientific constructs.

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# TEACHING METHODS RESEARCH

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THE problem of instructor- versus student-centered teaching is not a new one, although each time the controversy has appeared in print it has taken a different name! The disagreement has been further confounded because sometimes it has concerned itself with matters of content, and sometimes with matters of method. During the middle 20's, largely under Dewey's influence (6), the problem was investigated by those who were concerned with what came to be called "progressive education." Since this movement arose largely as a revolt, it took some time for its principles to be succinctly stated. In time, however, it became clear that progressive education was concerned primarily with the way individuals met and solved problems, with the habits they developed in adjusting to their environment, and with the implications of these for democratic living.

This controversy ramified into the realm of college teaching as the "lecture method" versus the "discussion method." The discussion method was felt to be more appropriate for a democratic society, for this method was supposed to encourage the reflective deliberation of problems. Furthermore, it was held that experiences in discussion were experiences in reflective thinking that could be observed and appraised in such a way as to stimulate growth. Therefore, in an era when educators held that it was more important to teach students *how* to think than *what* to think, the discussion method was thought to be superior.

Some research was conducted on the relative merits of these two teaching methods, and at least two reviews of the literature (5, 11) indicated that the lecture method was superior for purposes of immediate recall and more efficacious with superior students, while the discussion method was better for delayed recall, and for the less able students. In general, however, differences in definitions of "the lecture method" and "the discussion method," and differences in procedures make the results of the various studies difficult to compare.

Recently the problem has been re-opened under the title of "instructor-centered" versus "student-

centered" teaching (7), and much-needed conceptual and methodological improvements have been made in the research designs. The basic problem has maintained its continuity, however, for the instructor method is still more interested in course-related material, while the student-centered approach is primarily concerned with individual development and emotional needs. The reformulation of the problem, which has been largely influenced by Lewin's concept of the "interdependence" of needs (8) and by Rogerian nondirective techniques (13), has favored the student-centered method. This has been especially important, since the discussion method has never been clearly defined, existing mostly by contrast with its rival, the lecture method.<sup>1</sup> Essentially this restatement in Lewinian and Rogerian terms has served to integrate the teaching problem with the fields of group dynamics and psychotherapy, from which many important insights into the discussion and the lecture process have been gained.

Viewing the course of research in this area, an interesting paradox presents itself. One of the tenets of the student-centered side of the controversy has always been the importance of student personality development. However, when the results are seen, it is apparent that these researches have occupied themselves with methods which were supposed to favor individual development, but they have never really concerned themselves with the impact of the particular teaching methods upon the different patterns of students' needs. This criticism applies equally to earlier and to current research. There is considerable value in asking why

<sup>1</sup> A perusal of the literature, however, indicates that two quite different teaching styles are contained under the rubric "student-centered" instruction. The one kind, advocated by Cantor (4), involves an active challenging of the students' positions. Here the instructor is a kind of "intellectual sparring partner." In the other method (3, 12), which is more Rogerian, the instructor plays a less active, less directive role. Thus, although progress has been made in operationally defining the student-centered method, conclusions from different studies, both purporting to use this method, should be made with considerable caution.

some students learn better under one method of instruction than under another, and reformulating the student-centered-instructor-centered controversy along these lines may prove helpful. To this end we now turn. First, let us consider the results of one or two typical experiments.

A growing body of research is indicating the importance of the interaction between group structure and individual personality. Henry Maas, in a nicely conceived study (9), showed that leaders who tended to project blame improved their interpersonal perceptions when they led informal, unstructured groups, while leaders who tended to introject blame made similar improvements when they led formal, structured, groups. In the first instance the relatively unstructured nature of the group, and the activities they undertook, made heavy demands upon the leader, which, in turn, fulfilled his needs to manipulate external things, to interact freely with the group members, etc. In the second case, the group structure and function made sufficiently light demands upon the leader to permit him to remain peripheral as much as his own needs required; and the nature of the demands themselves was of such a "ritualistic" sort that they served to reduce a certain amount of anxiety in a way characteristic of these kinds of people. Since in both cases their needs were met by the group structure, these kinds of leaders made positive gains in their perception of the group members. Those leaders who were "misplaced"—for example, an introjecting leader in an open group—failed to make any improvements in their perception.

Maas was concerned with the way the group structure articulates with the leaders' needs. In a study already reported (15), we investigated the relationship of two teaching methods and several patterns of students' needs. Using categories derived from Rosenzweig (14), we found that intro-punitive students, who had high needs for structure, were extremely caustic in their comments about sections and instructors when placed in a "permissive" classroom situation. Similarly, students with high autonomy needs, who were extra-punitive, were extremely critical when put into a "directive" classroom. Students who were "satisfied," on the other hand, had relatively favorable attitudes towards the sections and the instructors.<sup>2</sup>

<sup>2</sup> Since this study was not designed to investigate this hypothesis specifically, it was impossible to obtain signifi-

These studies, which are offered as illustration rather than as proof, suggest that improvements in perception are greater, and frustration is less, in classroom situations where the students' emotional-intellectual needs are met by the prevailing group structure. Since diverse investigations (2, 10) have demonstrated that the frustrated organism is a poor learner, we may therefore hypothesize that in order to maximize the learning process, teaching procedures must be made to articulate with students' needs. This does not mean, as some have attempted to demonstrate (3, 12), that all teaching must be permissive in order to reduce anxieties. On the contrary, the studies cited indicate that students have learned different ways of reducing classroom tensions, and the best teaching method for *some* students is not the best teaching method for *all* students. The studies do suggest that in order to make teaching as effective as possible account must be taken of both classroom structure and individual needs. Thus, we would further hypothesize that students will learn best in classroom situations which offer means of tension reduction which are commensurate with those already fixed by the students.

This brings us to a second aspect of the student-versus instructor-centered teaching problem. This point is concerned with a clarification and a definition of the variables involved in this kind of research.<sup>3</sup> Many studies have been conducted which terminate with the findings that in student-centered classes there is free student-teacher communication, more student-student contact, etc. These are reported as results, and they are taken as the dependent variable. Now, if our problem is to assess the comparative merits of instructor- and student-centered teaching, a more logical analysis of the experimental design will show that such things as the amount and kind of classroom interaction is not the dependent variable. Note the proviso "if our problem is the assessment of the

cant information on the performance of these three groups of students on the final examination. This is certainly a next step.

<sup>3</sup> Other writers have indicated the importance of this issue. For example, Allport writes, "We come now to the most difficult question of all. . . . As in any other scientific experiment two sets of variables are needed: the dependent, whose change we wish to measure, and the independent, whose impact we wish to know . . . these . . . become the evidences of educational progress in the student and the style of teaching employed" (1, p. 41).

relative merits of these two teaching methods," for herein lies the confusion. If our problem is to assess these two methods, then the establishment of the right "group atmosphere" is not the dependent variable, but rather an aspect of the independent variable. Such things as equalitarian superior status contacts and enhanced peer interaction merely prove that the situation has been correctly defined by the students—to use W. I. Thomas's phrase. The students understand "the rules of the game." In more psychological terms, we can say that the instructor has successfully engendered a set of expectancies in the students for this experimental situation. This can be done, as Lewin and others have pointed out, and if the purpose of our research is to see if group atmospheres can be established and manipulated, then such variables as the various kinds of classroom interaction are genuine dependent variables. However, if our purpose is to determine the *results* of such group atmospheres, then, once the proper group atmospheres have been established objectively, our research must proceed to independent criterion measurements made under the two, or more, different classroom situations.

It is worth emphasizing that in this kind of research it is necessary to "prove" the fact that the independent variable, the group atmosphere, has been established. This should be done by the reports of trained observers, and by the reports of the students themselves—their "perception" of the situation. Only after it has been confirmed that the different group atmospheres do, in fact, exist, is one justified in making his tests, comparing his findings, and concluding that the two teaching methods produce different results. In this sense this kind of research is more complicated than studies where, for example, motivation is defined in terms of hours of food deprivation. And this complication may have led to the ambiguous uses of the independent and the dependent measures. In order to avoid further confusion in this area, therefore, we will attempt to indicate the value of clarifying the status of the variables involved.

It is possible to construct a paradigm which can put the principal variables used into their proper places. In so doing we can integrate the two points made in this discussion. The emotional-intellectual needs which the students bring with them to the classroom can be conceptualized, for one research purpose, as the independent variable.

These are then considered to be relatively stable, and no attempt is made to manipulate them. The teaching techniques are the intervening variable, and, as we have pointed out, the establishment of the group atmosphere, which is partly dependent upon the method of instruction, must be demonstrated independently. The dependent variable can be, then, the students' performance on some test of course-related material. This experimental model permits the independent manipulation of the three most important variables, which are generically conceived to be the students' needs, the teaching methods, and the performance on a criterion test. This way of conceptualizing the problem, however, may be applied to other hypotheses. For example, the group atmospheres may be considered the independent variable; the needs of the students may this time be considered the intervening variable, and some attempt might be made to manipulate them; and an examination over the course material could be the dependent variable. Thus, this paradigm permits of various different research purposes, and it has the advantage of writing into the experimental design all of the most important variables, each functioning in a given experiment in a definable and specific manner.

#### SUMMARY

In this discussion we have attempted to indicate some of the historical roots of the student-centered versus instructor-centered college teaching controversy, and have tried to show the influence of this historical background on the present status of the problem. We have emphasized the necessity for making teaching methods commensurate with students' emotional-intellectual needs in order to make learning maximally effective, and have suggested a way of conceptualizing this process which clearly defines the three most important variables.

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## CONVENTION IMPRESSIONS

DURING and after an APA convention a favorite topic of conversation among psychologists is the convention itself. The annual gathering of the clans is a high point in the life of many psychologists. We thought it would be interesting—and maybe even useful—to psychologists to collect written impressions of the meetings and publish them for all to see. Ac-

cordingly we solicited essays from a number of people, each of whom we thought would view the convention through a different pair of spectacles. We did not succeed in getting all the pieces we asked for, and some of those we received did not have the flavor we predicted, but the final array is presented herewith.—Ed.

### THE PRESS LOOKS AT THE 60TH ANNUAL CONVENTION

How did the 60th annual meeting look to the newspapermen who covered it?

The writer was in charge of the press room, a small pressure chamber generally unnoticed in the heat and blast of the mammoth boiler room which is an APA convention.

There, from 9 A.M. until about 7 P.M. each day, psychologists met the press. And on two or three occasions press and professor met head-on, and recoiled from each other, bruised like two people who have simultaneously bent over to pick up a piece of paper from the floor, and had their heads knocked together with velocity.

But generally they met in amity, as was verbalized (said, that is) by several reporters. From the viewpoint of the press an APA meeting is now judged "a good show."

As this is written, on a Sunday morning in the Hotel Statler, after the tumult has died, the "show" metaphor seems to have special validity (and, reading this note some time later, I note the phrase "three-ring circus" occurs more than once in the other contributions to this symposium).

This Sunday morning, the hotel officials who had been so brisk and hearty when welcoming the arrival of APA advance committees are now merely brisk. They are occupied with other advance committees for shows on which the curtain will rise next week. Before us, it had been the Greeks, a convention full of Greek-Americans with names redolent of Plato. After us will come the gerontologists—and the wholesale hardware men. The posters and banners of the APA are tattered now, or coming down from the walls. And the char-women, as inexorable as time, are giving the place

an exhaustive cleaning. These ladies start from the walls out, for apparently the blast of the APA boiler room has left a thin film of tobacco smoke, no doubt congealed with jargon, on the murals and the panelling around the Presidential and Pan American rooms, and the other innumerable council chambers.

There is a distinct flavor in the air of "After the Ball is Over," and it seems a good time to ask "What does it all amount to?"

So far as press relations of psychologists and their profession are concerned, there is something valuable simply in this: such an ebbing and flowing congregation (or convulsion) gives the press a chance to meet a lot of people called psychologists. As many professors say they go to the meeting "mainly to meet the people," so it is valuable, I think, for the press to meet real, live psychologists individually, *en masse* and *en passant*.

The press finds psychologists easy to talk to and comparatively easy to listen to—they are probably not so extraverted as Rotarians, but certainly at convention-time they are a far cry from the stereotype of the "mousy little professor."

Perhaps we can enter in the record one statement not from a member of the press, but from another seasoned observer of humanity—a bellhop. This morning a wise-eyed minion of the Statler, who did not realize I was a leftover from the convention, delivered himself of the following observation on these Ss:

Well, it was a good convention. You know, no trouble. Like all of them, they never go to bed. Two o'clock in the morning, still crying for ice. Well-behaved, though. I don't know of a single row with the help. Of course, they

like to drink a lot. But they all do. That is, outside the D.A.R. Those ladies don't drink at all. But *these* boys—what talkers! Not so much singing but talk, talk, talk, all night, and at nine o'clock in the morning the lobby is like the third round at a cocktail party.

Now newspapermen, as academic people may not be aware, are as fond of talk as any class of people in the world. And the thing about an APA convention, as distinguished from certain other stiffer gatherings—say, of the military, the reminiscent-military (no names, please), or big business—is that the APA participants are approachable, they will talk, and they will talk with a minimum of cant.

So one might have predicted that these professional listeners, newspapermen, would have a pleasant time at the convention with some other professional listeners (psychologists), who for the week had been turned loose to talk.

It might be noted, however, that some of the reporters complained about some of the current jargon. This is serious, for reporters get around—they are familiar with the government man and his "implementation" and "policy directive." They know the adman with his "socko packages" and "sizzler with plenty of sell." It is thus remarkable that they find psychological talk remarkable.

But today psychologists use so many words which have ordinary day-to-day meanings; they use so many which are not even agreed upon within the profession, and they use so many really hideous words like "constructs," that one can not help feeling there is a special horror about their language. Is it inevitable? Psychologists in the past have given us so many self-illuminating phrases, such as "conditioned reflex," that perhaps one must not give up hope that psychology is now going through a phase from which it may emerge with a clearer language reflecting a clearer self-knowledge. Or should we say, "Further exploration and amplification of semantic constructs may yet be meaningful, even resultful in dissipating interpersonal communicatory confusion, thus creating a greater integration of reciprocal relating in face-to-face relationships."

Sometimes reporters were amused with the psychologists they met, for there are still many reporters and many citizens in the world who find the special qualifying titles of scientific papers just in themselves amusing, and likewise they find the

careful specifics of professorial explanations amusing.

As for the first, to the average reporter, a good many of the titles in the big blue program read to him like so: "A Study of Radiation-Induced Color-Blindness in Left-Handed Albino Penguins."

(I note with pleasure that some of the foreign visitors to the convention also complain that there are not more rigid standards as to what may be presented to APA, and also that they feel there should be more major papers of a synthesizing or philosophizing nature.)

As to the latter, there is another thing to be said. The talk of psychologists, one reporter told me, is quite different from that of other academic folk, in general terms. The differences, she said, come from the fact that by the nature of their studies psychologists are not so sure of themselves and, therefore, do not seem, to the press, to be so "arrogant." Perhaps psychologists are not so sure of their own minds, merely because they know how difficult it is to be sure of anyone's mind. No doubt some, reading this, will ask, "What do you mean by mind?"

Anyway, it is amusing to find this self-doubt, and amusing to find that psychologists do indeed talk among themselves just as cartoonists (and writers) and other light-minded people rather hoped they would. (You're fine, how am I?)

It turns out—as a psychologist might say—it turns out that psychologists *do* preface remarks with, "Well, perhaps it is just my neurotic honesty, but I feel . . ." They do tell you about someone you are going to meet, and say, "Now you'll find him an anxiety type, with a rigid approach. . ." (In the latter case the introduction then rapidly fell apart, for the speaker added, "But basically he's a real nice guy and a lot of fun on a party.")

Thus the press found the psychologists—rarely arrogant, frequently serious or overserious, sometimes amusing, nearly always approachable, and always talking.

And, if you took time to read a newspaper during the meeting, you saw the result was that a great deal of psychologists' talk found its way into newsprint. But that is another story.

MICHAEL AMRINE  
Public Relations Consultant  
to the APA

## CONVENTION EXPERIENCES, OR OH FATHER, HOW YOU HAVE CHANGED

Dr. Sanford has asked me to contribute to this collection of impressions of the 1952 convention of the American Psychological Association. Apparently, I have been chosen as the "Old Timer," which is certainly appropriate. I attended my first meeting in Washington in 1911, the year before I was elected to membership. If my memory does not fail me, the recent convention was the twenty-fifth meeting that I have attended. The first time that I read a paper was at the Philadelphia meeting in 1914. And I want to establish, from the start, that my mention of changes does not mean to imply that these changes have not been for the betterment of psychology in general, and for the Association in particular.

In the first place, one becomes more and more impressed with the large numbers attending the meetings. This is simply a corollary of the growth of the Association. At the recent meeting, there were over 4,000 registrations, and who can estimate how many more were present who did not register. In contrast, when I became Secretary in 1926, there were fewer than 500 members. In other words, 26 years ago the total membership was less than one-eighth of the members registered in attendance at the last meeting. It will be remembered that the first election to Associate membership followed immediately the adoption of the necessary changes in the By-Laws in 1925. The 1951 Directory lists 69 Life Members, 1,506 Fellows, and 6,979 Associates for a total of 8,554, or more than 17 times the total membership in 1926.

This change in numbers has led to many changes in the form of the annual gatherings. These used to be called meetings, and they are now properly called conventions. The old timers find the rooms, and especially the corridors, far too crowded. And this is especially true inasmuch as few academic institutions can house such a meeting—it has assumed more or less the hotel-convention pattern. In the old days, all meetings were not only held in academic buildings, but rooming space was provided in student dormitories.

Also, in years past, there was an annual dinner attended by all members present who had the price, and followed by the Presidential Address. All of this made for better fellowship in the old days. Almost everyone knew almost everyone else at the meetings. New members and students were

taken in tow by their professors and most carefully introduced to the older members present. Obviously this is not possible with the present crowded attendance. And, of course, another factor is the present great variety of interests and fields within the science. With the present specialization in psychology, one can hardly be expected to have a wide acquaintance with many individuals in remote fields. As the years pass, I am particularly struck by the smaller and smaller proportion of individuals in attendance who address me as "Ferny."

The time of the meetings has shifted from the Christmas holidays to the time between the closing of summer schools and the opening of a new academic year. This change was a valid one, inasmuch as the new time for conventions undoubtedly makes for a larger attendance. But the new time also makes for a difference in temperature. I can hardly remember being more unpleasantly hot than during the Toronto meeting in 1931. It is true that meeting in hotels has led largely to air-conditioned rooms, which will not be found in academic institutions. Of course, some will object to the cold of the previous meeting time, especially if they remember the Wisconsin meeting in 1923. And the meeting time has been extended from three days to seven days, or even eight or more if one considers the early meeting of the Board of Directors.

The reason for this extension is obvious. There are many more papers and meetings to be scheduled. I can remember the days when there was no duplication of programs. Then, most everyone who attended the meetings was interested in all phases of what was then considered psychology, and hence attended all of the sessions and listened to all of the papers. In 1926, fewer than 50 papers were read. In contrast, at the recent meeting (as culled from the program), there were 401 papers, 83 symposia, round tables, and discussions, 17 addresses (of which 4 were invited and 13 were presidential addresses), 58 scheduled meetings (including luncheons and dinners), and 7 film showings. This more than outnumbers by ten times the scheduled papers of 25 years ago. As an old timer, I am almost appalled when faced with the decisions of where to go and what to hear in the present set-up. But with our membership, and the

variety of interests among the members, such a program must be anticipated. But what will happen if, as, and when our membership continues to grow at its present rate? The officers and Program Committee are to be complimented on arranging shorter individual programs, usually with only four papers, a pattern which gives greater opportunity for circulation among different presentations.

I am also impressed, both with looking at the program and chatting with some of the officers, with how the Association has gone into big business. As Treasurer, I served for nothing and was amply paid for the small amount of work asked of me. As Secretary, I received a small sum which at least helped to pay for a part-time secretary. As editor of journals, I received a sum sufficient to pay for necessary postage and for some small amount of secretarial aid. Today, and again because of the large membership, this sort of volunteer contribution becomes more or less impossible, except, I hasten to add, for the Board of Directors who seem to meet more or less continually during the time of the convention. The result is a necessary concentration of executive power, primarily in the office of the Executive Secretary. And the policies of the Association are now determined, not by the total membership, but by elected representatives. The annual business meetings used to be amusing, and they no longer exist.

#### RECOLLECTIONS OF THE CONVENTION

When you make your home just twenty-six miles from the Canadian border, your recollections of a convention held in Washington, D. C., during a hot and humid week are inevitably colored by the struggle to adjust to the weather and the hotel air-conditioning systems. I must confess I lost both struggles. It is not strange, then, to find that most of the unpleasant incidents that come to mind are somehow related to that ordeal of heat and sudden drafts. The endless trips between the Mayflower and the Statler, the crowded reception hall, the packed social gatherings, the three-shirt-changes a day routine, and the contrasting, icy calm of half-filled meeting rooms are among these negative recollections.

There remain many positive impressions to balance these memories of physical discomforts. I enjoyed seeing old friends, meeting people who

Let me indicate an example of the informality and the fellowship that existed in those days. At an annual meeting, the question of the change of meeting time was up for discussion. My very good and old friend Professor Dallenbach and I led the arguments on opposite sides. When a vote was called, it was obvious that a division was required. And, to the amusement of all present, the president appointed Dallenbach and me as tellers. Incidentally, Dallenbach won and the meeting times were changed from the Christmas holidays to sometime about Labor Day.

Now all such decisions are made by the Council of Representatives and, although there is a report by the Recording Secretary late in the convention period, few of us know what has been decided until we read the November issue of the *American Psychologist*.

In summary then, the conventions of the American Psychological Association have gained tremendously in importance and, as large conventions, they have been admirably administered. But, to an old timer, they have necessarily lost the close contact and the fellowship which was once such a marked characteristic of the earlier meetings, and again I draw the distinction between meetings and conventions. Indeed one can say, "Oh Father, how you have changed!"

SAMUEL W. FERNBERGER  
University of Pennsylvania

had previously been just names in texts and articles, having dinner with a former professor, hearing some excellent papers, and attending several stimulating panel discussions. It was particularly gratifying to meet and be remembered by some of my own former students.

On the "professional" side, I found the opportunity to preview films and the conferences on personality factors in vocational selections most rewarding. It seemed to me that some of the panel discussions would have been more successful if two, or at the most three, persons spoke. As it was, I found that sessions purporting to present different points of view turned out to be a series of somewhat unrelated papers, offering five or six shades of opinion—not necessarily on the same issues. The question periods after such panel presentations were, quite naturally, confused and rather frustrating.

The Washington meetings reinforced my feeling that conventions often neglect the obvious desire on the part of its members to use much of the time for informal meetings with the friends and acquaintances we see only on these infrequent occasions. Torn between the formidable listing of meetings and the desire to exchange a few words with a colleague, the net result is a reduction in the satisfaction from both of these important functions of a convention. I frankly do not know how the arrangements committee can resist the pressure for more and more papers, more and more conflicts in the schedule of events of interest to the same people, and the tendency to schedule the time for the sessions so as to encourage "meeting-hopping." ("I'll try the first part of this session and then the last part of that one.") I expect that better physical arrangements for the informal chats I consider so important are equally difficult to arrange. Dividing the meetings between two hotels and the housing into many segments is, probably, an inevitable consequence of the growth of our organization. The bigger we are, the harder we meet!

While at the convention I assisted our dean of the college in interviewing and selecting a new member of our psychology department. It seemed to us that some practical improvements could be made in the placement service arrangements. Greater privacy for interviews, less haphazard facilities for exchanging communications, bulletin board listing of job openings, a coded bulletin board listing of applicants, and provision for typing service (paid, if necessary) for both applicants and employers would provide valuable improvements in these placement arrangements.

#### APA MEETINGS—TIME FOR A CHANGE?

The 1952 meetings of the American Psychological Association have followed the same pattern as past meetings, but there have been more of them. Even without quantitative analysis, the 1952 program number of the *American Psychologist* seems to have more pages and weigh more than any previous number. With the continued rapid growth of the Association, more members will want to present more papers. Extrapolation makes the prospective number of future papers appear alarming.

I wish to propose some marked changes to overcome what may be the effects of tradition on the conduct of the meetings. The most important

As a final note, I should urge care in speaking to strangers. The most vivid, but certainly a sobering, recollection of the APA convention is the nationwide press and radio coverage (distorted, of course) given to a few casual remarks I made concerning the possible effects of television on children. At a luncheon meeting a representative of one of the news services made note of my impressions gained from numerous contacts with high school counselors in television and nontelevision areas. I expressed doubt as to the accuracy of the "television will create monsters" theory, indicated that the high school counselors in the television areas reported no unusual personality symptoms attributable to television programs, and observed that recent developments pointed toward a greater use of television as an aid in the educational process. The next day one of the Washington, D. C., papers had a "version" of my remarks on the front page. Wire services spread the item to newspapers and radio stations throughout the country and clippings poured into the public relations office of the State University of New York. Subsequently, I received urgent requests from the APA, from the State University, and from a committee of commercial television and radio broadcasters to furnish copies of my "research study" and the "paper" I had delivered at the APA convention. In fact, I barely avoided testifying at the Congressional hearings on the effects of television and radio the very next month.

But, despite the heat and confusion, it was an interesting and worth-while convention.

DAVID D. KOMISAR  
*Champlain College*  
*State University of New York*

function of the APA meetings, as I see it, should be to present for those at the meetings, and for future use, the major trends, changes, and innovations in psychology during the year. Each yearly meeting should be a unique landmark which would allow an annual accounting in the various areas of psychology. The programs should be well organized, meaningfully integrated, and interestingly presented. This would place in proper perspective the excellent work being done by many psychologists at the meetings.

With these purposes in mind, only a relatively few well-planned meetings dealing with the most

important topics of the year would be needed. Could not these meetings be organized to fire the imagination of psychologists, to stimulate them to the best possible work? What psychologist would expect groups to learn much if he presented material in the manner in which he is exposed to it at APA meetings? Each meeting could be concerned with significant, major problems in research, teaching, professional practice, or other important areas of psychology. The most modern methods of presenting materials meaningfully and of obtaining audience participation could be utilized.

Each program committee could review and evaluate the year's work with a view to selecting meeting topics. A committee could be aided in this task by various official APA and divisional groups and by membership polls conducted to elicit information on current work, possible participation, and other matters. The topics would be of such stature that meetings would require half or full days. Not more than three or four meetings should be held at any one time; large meeting places would be needed. Three to four days should be ample to cover the program. Evenings could still be used for presidential addresses, business meetings, and other activities.

These changes would produce few if any losses. The research reports as now given are usually too brief to be fully appreciated. Most of them are published, at which time they may be much better understood. On the other hand, most of the lectures and symposia are not published, with the result that the opportunity to make long-term contributions is lost. Fewer individuals would participate in the actual programs, but relevant important contributions could be integrated in the presentation. Audience participation would involve more people. If a need is felt for the individual presentation of brief research reports, this could be dealt with at regional meetings.

#### AN INNOCENT ABROAD IN WASHINGTON

Within the space of thirteen months, I have experienced the mixed pleasures and pains of three national conventions of psychologists. The first was a cozy little affair held in August, 1951, in Melbourne (a city of about one and a quarter million people) by the Australian Branch, British Psychological Society. About a third of our membership of almost three hundred attended. Almost everybody knew almost everybody else; only

What values could accrue to psychology by making these changes? The meetings could represent the very best work and thought in American psychology. Audiences could be given genuine intellectual stimulation. Such a high quality of psychological endeavor need not be lost to posterity. All the sessions could be published. Sound recordings and movies could be made so that the meetings could be used for both classroom and historical purposes.

The integrated, meaningful picture of psychological work presented in these meetings could do much to increase understanding and appreciation of psychology by the American public. Perhaps better integrated meetings would facilitate adequate news coverage. Some of the meetings, when held as envisioned here, could be broadcast or televised, with resultant educational benefits.

Meetings such as these, dealing with major, important problems each year, could act as unifying forces in psychology. Overspecialization, splintering groups, and schisms plague the development of psychology. Psychologists have little opportunity to look at psychology from broad points of view, to see their relation to the over-all subject, and to see how the various kinds of professional work and research fit together. Problems arising from overspecialization can be seen in some other professions. It is certainly not too early to make definite efforts to counteract such trends in our profession.

This is only a brief outline of a plan that would need a great deal of work before it could be put into operation. More work would be needed to make it operate successfully. The results would be worth it. Each year, the APA meetings would write important, significant pages in the history of psychology.

SIDNEY H. NEWMAN  
U. S. Public Health Service

occasionally were there alternative items on the programme; all in attendance could be accommodated at the annual dinner of the Branch; and it was possible for the Melbourne members to play host at a party (beer, of course, being served) for the rest of us. Quite a few who had offered to read papers were rejected by the Programme Committee so that adequate time would be available during the five days for the presentation and dis-

cussion of those papers which appeared to deserve the favor. Alternatives were set down by this committee largely when it was felt that each of them was so specialized or technical as to appeal to non-overlapping audiences. No doubt the committee made some mistakes of judgment on both points. The second convention was held in Oxford, April, 1952, by the British Psychological Society. It was much bigger than the first and had necessarily to sacrifice some things in order to gain other advantages that come with size. Some four hundred members attended and it was clear to a visitor that almost everybody did not know at least a big minority of his confreres. For a good half of the time, the "two-ring circus" was in force, and in general the time allowed for speakers was less than in Australia, as was the time for questions and discussion—there were quite a few papers that had no more than thirty minutes. However, there were still many sessions in which the experimentalists, the psychometricians, and the clinicians were together, and so were enabled to come into head-on collision with each other. Perhaps, because these different varieties of psychologists see less of each other during the year than do their counterparts in Australia, these collisions were a little noisier and a little more violent than those to which I had been accustomed.

And then came the deluge! The many thousands of psychologists present in Washington and the many more thousands held as it were in reserve at their home bases, suggested to me a new yardstick with which to measure the relative standards of material and technological culture in the three countries of which I now have some firsthand knowledge. Up till then I had been accustomed to think of the relative numbers of automobiles, of telephones, and what not per thousand persons in America, Australia, and Britain (I set them down roughly in order of magnitude on these comparatively unimportant things in life); but in the future, I am tempted to make these comparisons in terms of the number of persons per thousand psychologists. The deluge took other form too. Upon receiving the July issue of the *American Psychologist*, I painstakingly worked through the many offerings, trying to make my choices in advance. My frequent donkey-like vacillations between the some half-dozen bundles of succulent-looking hay led me to formulate a theory about those who had told me they went to the convention

site solely to meet their friends. I supposed that the self-same conflicting vectors now operating upon me served to drive them from the field. Later I formulated another hypothesis.

But although overwhelmed by so many friendly people and so many attractive looking items on the programme, I found the convention an exciting and rewarding experience. Part of this derived from the opportunities for attaching faces and personalities to the many names so well known through the texts and journals upon which we rely so heavily in Australia. Even more important were the opportunities for discovering the current thinking of some of these well-known people, or for discovering that some of them were not currently thinking. For these two objectives, the symposia and the addresses on the program and the private talks off it were the most effective means. But underlying and detracting from this excitement and reward was considerable frustration, arising most intensely from the clotted mass of short papers, some sessions of which went near to producing in me an audiogenic seizure.

My most general judgment of the convention was that this is a grand institution that you have, but that it is badly in need of a purge. The primary congestion is not caused by those who sat or stood around in the lobbies while sessions were in progress. They did nobody any harm and gave pleasure to many. The matter simply is that there is too much on the program, too much not deserving a space when space is so precious. This diagnosis can best be illustrated by reference to my *bete-noir*. Although the attempts to group the short papers in terms of probable audience interest were often ingenious, the groups too often were made up of items unrelated in real theme and uneven in quality—little gems and bits of lusterless glass side by side on the string. And when one had the not infrequent luck to encounter a gem and wanted to inspect it a little more, to turn it, as it were, from side to side to catch a little more of the real light in it, it was suddenly snatched away and too often a bit of glass was thrust into one's hands. Though I was subsequently glad that retroactive inhibition helped one bit of glass to push another from my memory, I resented its equal action on a gem. This demand for more extended treatment of a topic springs no doubt from a cultural difference which you may not share with me; but at least I can rationalize my prefer-

ence. For one thing, paper-and-print is the best medium for cut-and-dried presentation, whereas spoken presentation is best when the listener can and does reply. And again, though sixty minutes of poor stuff is more than four times as unbearable as fifteen minutes of the same stuff, there is much to be said for giving a man enough rope, even if he only succeeds in hanging himself.

Another product of the congestion is the thorough-going breakdown of the program by divisions, which is admittedly very efficient for some purposes. But surely, American psychology has not yet developed to the point where only the President of the APA and one or two others can find something to say that is presumably of interest and concern to almost everybody. This so thorough divisional segregation acts against the clashes of opinion that so delighted me at Oxford. Keeping sects apart helps keep the peace, but it does not help either to resolve their differences or to sharpen them to the point where what is to be resolved may be seen. Because the symposia did enable some differences to emerge frankly and clearly and because they did not demand a mental jump every fifteen minutes from crag to crag, or to abyss, these sessions ranked high on my list. And this evaluation holds even with the admission that the material in some of the symposia was lighter weight than that in some of the better short papers.

The remedies that arise from this diagnosis may prove hard medicine both to give and to take. There are many who want to give a paper and rigorous selection is difficult. So I wish to suggest an easier if less effective remedy. It involves an extension of the divisional breakdown—a kind of killing a fever with a fever. Two additions suggest themselves—21. Division of Free-Riders, and 22. Division of the Slave Market. The former would cater to those who give papers because by doing so they qualify for a contribution to their convention expenses from their employing institution. And the platform of the latter would be reserved for those who wish to exhibit their biceps to prospective employers of their services. It may

be that old convention hands were more expert than I in smelling in advance papers of these two sorts masquerading in one or another of the existing divisions; but, possibly they were not and decided it was safer to stand around in the lobbies.

If in some way or other, these trivial papers could be eliminated from the program, the more serious, scientific papers could be given the time, the consideration, and discussion they deserve and need. This is not censorship that is being advocated, it is merely some respect for standards without which any scholarly, any scientific endeavor cannot prosper. The APA, especially in its convention (but elsewhere too), faces the issue of quantity *versus* quality. As things stand, quantity threatens to overwhelm quality. There is a lot of life in this giant frame, but it had better not forget what happened to the dinosaurs. Indeed, if Divisions 21 and 22 are created, their members should be encouraged to publish a journal. Members of other divisions should as a moral duty subscribe to it (and file its issues unread). Financial success thus guaranteed would ensure continued existence of the journal, which in turn would bring perpetual relief from strain upon the other journals. If, as it seems, the members of these divisions-to-be are making the American psychologist a reluctant listener, they may yet make him as reluctant to read his own journals as he is to read those published overseas.

Knowing that the convention was not made specifically to my measurements, I have no personal complaints about its failure to fit snugly all the regions of my (perhaps odd) intellectual anatomy. But suspecting that quite a few APA members have not entirely dissimilar dimensions, a complaint is being lodged on their behalf. I enjoyed my first (and I hope not only) APA convention—the foregoing remarks are merely a strange, antipodean way of expressing my appreciation to the APA for its hospitality to me in Washington, D. C., September, 1952.

W. M. O'NEIL  
University of Sydney, Australia

#### PSYCHOLOGISTS BY THE THOUSANDS

Have you ever worried about over four thousand psychologists of varying sizes, shapes, sex, with differing purposes, ideas, identifications, and perceptions? Your colleagues in the Washington-area did. Acting as the Local Arrangements Commit-

tee for the 1952 Convention, about 40 psychologists worried, worked, and wished over the convention proceedings and events for about six months before the meeting. During the preparations no one close escaped without a chore to be done.

Husbands, wives, children, secretaries, and graduate students were drafted. The 40 individuals on the Committee who gave of their time and talents so freely worked that all details of a large-scale convention would be handled easily and effectively. They worried that room arrangements, meeting plans, signs, dinners, etc. would come off as planned. And they wished that each APA member attending these meetings got what he wanted from his participation and had a good time.

One Committee member had an interesting experience trying to predict the sex of a psychologist from the first initial of that person's first name. It was a difficult judgment, and he was required for room reservation purposes to judge in two categories. Apparently, everyone was satisfied with the judgments made. But the Housing Bureau kept calling to ask what was the sex of the person whose first initial was "L."

Can a convention be managed from a D.C. police precinct house? It was done (that is if managers really manage) when the Convention Manager was held for several hours for speeding on Labor Day during a most effective safe-driving campaign. An hypothesis available for testing in the future is "the convention runs better when the Convention Manager is in jail."

Can anyone ever really predict how many psychologists will get up out of bed on a Thursday morning and attend an 8:40 A.M. meeting on some esoteric research area? Such predictions were made and probably the usual number of mistakes occurred. We assigned a room seating 1,000 for what turned out to be a select group of 20, and assigned a room holding 50 people to which 500 were attracted. Most of the predictions, however, were really good.

The Committee had to find a room for an *important* meeting of thirty *important* psychologists on an *important* committee in the next ten minutes in a series of rooms already scheduled for the next three days. Psychologists are inventive. Our Room Assignments person found space.

The Mail and Directory group never before realized how busy and important psychologists are. Even after the psychologist leaves his base of operations for three or so days, he leaves behind him a trail of U. S. mail consisting of assorted sizes of packages and letters of varying degrees of urgency. Anyone would be impressed with the ingenuity of

our colleagues in using scraps of paper, corners of envelopes, white shoe strings, shirt collars as places upon which to write notes for Joe to meet him! We must express our regrets for not foreseeing the need for more appropriate and extensive mail facilities.

At the same point in time the Committee had to do (among other things) the following: find a black bow tie, supply the name of a lawyer, find some books, locate somebody's husband, count money, sell programs, make signs, change rooms, offer directions on how to get into the city and out of the city, provide guides for tours of the new headquarters, etc.

If anything was demonstrated by the behavior of the 40 or so psychologists responsible for the convention, it was their great enthusiasm, skill, and good sense. In the future we will all be very happy to attend conventions in any other city of the country, and appreciate the "behind the scenes" efforts involved.

The Committee made an assumption about nearly 5,000 people and faced the very disconcerting situation of having the usefulness of this assumption tested in the very near future. We assumed that psychologists would act intelligently and in a socially responsible manner, if we provided them with pertinent information on which to act, rather than do the tasks for them. It was our guiding philosophy to help people do what they wanted to do, but not to do it for them. Apparently, this assumption about the behavior of psychologists is one which we might have had no fear in making. Psychologists are all to be congratulated. We can report no broken bones, broken chandeliers, or even minor damage to physical property of anyone concerned. No one has grounds to complain much about psychologists' convention conduct.

Before closing, we might consider a little more seriously some continuing problems of our conventions which are due to grow in size and importance. Why do psychologists come to a convention? What do they do? What do they want to do? With the help of many people we have been able to make a start in answering these questions. Father J. W. Stafford (Catholic University) and his students were able to run a census of attendance at the various portions of the program. The APA Committee on Questionnaires under Ray C. Hackman, with the help of a group

of VA clinical psychology trainees, tried to ascertain the feasibility of interviewing members during the convention, studying attitudes towards the contents of the program and the usefulness of the abstracts issue of the *American Psychologist*. An *Ad Hoc* Committee on the Convention Program has been called into action. From this group should come some pertinent suggestions on what should take place during our conventions.

The Local Arrangements Committee was glad to have you here in Washington. We hope you had a good time, met some fellow psychologists you didn't know before, renewed old friendships, heard a good idea or a good paper or a new story, and came away impressed with the growth of psychology.

SHERMAN ROSS  
1952 Convention Manager

#### THE GREATEST SHOW OF THE YEAR

Americans are now quite accustomed to the astonishment of their western European visitors at the vastness of their country, its various enterprises, its educational systems, its hospitality. But even for an American, three years abroad is sufficient to require some considerable re-adaptation to the grand scale on which affairs are organized.

The size of the program arranged for the APA's Washington conference is an awe-inspiring sight to members of western European psychological societies who have only recently agreed—with considerable reluctance—to the scheduling of parallel programs at annual general meetings. With so many choice morsels dangling before him, the visitor may vacillate, as an intellectual Buridan's ass in the throes of conflict, wondering which program to attend. But with a little coaching he may learn the local method for relieving tension by leaving the field and seeking his enlightenment in the company of other fugitives at one of the inevitable "gatherings" that distinguish APA meetings from the more sober, or should I say "sombre," conferences of his native professional society.

The size of the APA and its annual meeting could not help but lead to specialization of interests. Members of smaller societies attend their annual conferences expecting to listen to speakers from many fields. Indeed some feel that this is an opportunity to discover what advances in knowledge, what points of view, what new methodology those in other fields have now to offer. With large numbers of papers in particular specializations it seems to have become much easier for APA members to rationalize themselves into attending discussions of their own specialty or its very close-of-kin only.

The visitor who seeks to find the ground upon which the impressively large structure of American psychology is founded cannot fail to recognize that the profession enjoys a much fuller ac-

ceptance by society in that country than in his own. Although the lay American may be as confused in his everyday "use" of psychology as the lay European, American psychologists appear to have convinced at least certain important social institutions that the psychologist can make significant contributions in American society. Psychology in many other countries does not enjoy a comparable reputation. The consequences are that the struggle for recognition necessary to the healthy development of the profession is still a very persistent and time-consuming reality. The far-reaching enterprise of American psychologists was strikingly evidenced at Washington, not only in terms of the wide range of subjects discussed but also in the special meetings of persons associated with specific industrial, clinical, military and other social agencies.

In addition to other advantages, acceptance by such agencies has meant extensive financial support for psychological research. The extent of this support as evidenced at the 1952 meetings is far beyond the European psychologist's capacity for imagination. It is true that agencies providing financial support for research in Britain are at present very sympathetic in their considerations of requests from psychologists, but they are few in number and limited in funds. The more extensive financial support of psychology in America is one of the factors contributing to the position of world leadership in psychological research which American psychologists have now achieved. With such a position come certain responsibilities and it is encouraging that American psychologists on the whole, as evidenced in their discussions of research programs at Washington, have not dashed off in all directions chasing *ad hoc* problems, but have insisted on the importance of systematic attacks on psychological issues at the level of fundamental research. The potential evil of channelling re-

search to fit the limited interests of supporting agencies seemed to be less of an actual worry than might have been anticipated, despite the occasional observation that a psychologist was not carrying on research of major interest to him but rather research which he believed was more likely to receive financial support.

Through what may indeed be somewhat jaundiced eyes, European psychologists not too infrequently view certain of their American contemporaries as overemphasizing technology without having achieved the strength of scientific foundation to support it soundly. Concern over the possible dangers inherent in such a situation could be recognized as appearing fairly frequently during the Washington meetings and it reached threshold value in the discussions of certain groups. Examining in retrospect certain undercurrents discernible in the flow of last September's events leaves the strong impression that here is a matter which is likely to appear persistently in the future affairs of the Association.

It has been rumored in European psychological circles that American psychologists are plagued by the dictum "publish or perish." The quantities of American publications in psychology during the past several years of expanding research opportunities have been interpreted by some as evidence in support of this rumor, and the visitor to Washington last September might have concluded that what he heard and the book exhibits he saw did little to contradict this impression. It is *not* true that *everyone* is writing a book, but it somewhat puzzles the impecunious western European visitor as to who buys all the books that *are* written and

how his American friends find time to decide which books are worth-while reading.

This 1952 meeting left a definite impression that American psychologists are conscious of and interested in their responsibilities to society as well as the contributions they can make to its welfare. Those who attended the meeting learned that in facing these responsibilities American psychologists, through the APA, have undertaken to investigate systematically such matters as professional ethics and standards, training at undergraduate, graduate, and special school levels, relations with other professional groups and with the general public. As a society they saw fit to express publicly their disapproval of incidents involving racial discrimination. As individuals and through their appointment of a committee on international relations in psychology they showed a concern over professional matters which went beyond national boundaries, a concern which is welcomed by many psychologists in other countries.

With little fear of contradiction it can be said that the 1952 APA Annual Meeting was the greatest show of the year in psychology. If there were any really dull moments, the spectator can only blame himself for not shifting his attention to another ring. As is characteristic of all complex stagings, no amount of effort on the part of any single observer could ensure entirely that he would not miss some of the spectacular acts. For anyone who was taken in by the freaks it can only be said that "suckers" may be born even among psychologists.

ROGER W. RUSSELL  
University College, London

#### THE CONVENTION AND THE YOUNGER PSYCHOLOGISTS

Many APA members, especially on the last days of an annual meeting, start reminiscing about the good old days when meetings were small, intimate gatherings, held in the quiet dignity of a college campus. But just as many, and perhaps more, of the younger psychologists never saw those halcyon days. The only meetings we have attended are large, hectic affairs. The only program we know is the one crammed with papers and symposia and business meetings and social affairs. This is what we expect of an APA meeting. But what do we think of it? Although I can speak only for myself, here are some of my reactions to the last meeting.

While the meetings are certainly a valuable experience, it seems to me that many of the benefits come, not from the convention program itself, but from other parts of the convention. The placement service, for example, is most helpful to anyone who is either interested in changing jobs or wants to get a general idea of the kinds of jobs which are available in psychology. At the annual meetings we also have an unusual opportunity to meet other people in the field and to exchange ideas with psychologists whose interests and areas of work may be similar to ours. However, one's first APA meetings are somewhat bewildering. I think that academic advisers who encourage

younger people to attend APA meetings should take the responsibility for seeing that the younger people do have a chance to benefit from them. It is sometimes difficult for a person attending his first APA meeting to know which parts of the program will be interesting and worth while unless someone is willing to advise him.

Although the opportunities to meet other people in the field of psychology and to learn more about the field itself are certainly good criteria for evaluating the APA meetings, I believe they should be considered secondary to the actual APA program itself. I have heard some general criticisms of the kinds of research reports which are made at the meetings. In many cases these papers are devoted to research findings which, because of the necessarily short time which can be devoted to them, are reported out of context in terms of relationships to larger research projects. We hear about research that is not particularly meaningful in itself, but which gains meaning only when reported in terms of finding the answer to some larger psychological question. A young psychologist coming to an APA convention and listening to all of the papers gets the impression that psychological research is made up of a series of small unrelated projects, without any particular relevance to anything else that is being done in the field. This seems to be a valid criticism, even though it can be assumed that those people attending APA meetings have a common knowledge of psychological progress. It would perhaps be better to bind the various elements of the program into a more or less unified whole. One way in which this might be done would be for each division to select a theme which would provide a framework into which the research papers could fit. This might mean losing some of the present breadth of subject matter which is now touched upon. However, younger psychologists in particular would probably find that the meetings would be more helpful to them if they included a more comprehensive coverage of one aspect of psychological research and thought in each division, rather than a series of relatively unrelated research reports.

It is also possible that the kind of research papers read at the meetings would be more meaningful if they were presented in written rather than oral form. This raises the question of the kind of content which should be included in the meeting. Perhaps panel discussions and seminars, which

could not adequately be replaced by written reports, should be emphasized. Since attendance at conventions has become so expensive, perhaps the APA convention program should be limited to the kinds of programs which are effective only when presented orally in a meeting.

This question is closely related to that of the selection of psychologists to participate in the program. There should certainly be a place for participation by younger psychologists who have not yet made a series of contributions to psychological knowledge. However, many of us would prefer to hear more from those psychologists who have "arrived" as leaders in the field. Perhaps the time has come to consider a place on the program as recognition of outstanding service to the profession over a period of years. A larger part of the total program could be devoted to fairly informal discussions of some of the questions which are still unanswered by psychologists. This, of course, would not eliminate the reporting of research data but would mean that the results of research projects would be reported by the experts in the area of knowledge under consideration, and would be reported as part of a larger discussion topic.

The suggestion to have those psychologists who have made outstanding contributions play a larger role in the meetings themselves would raise one serious problem. We would probably find that the people most in demand for the program would be the very people responsible for taking care of the Association business to be transacted during the annual meetings. Perhaps an answer to this problem would be to limit the program to two or three days with the rest of the time being devoted to organizational business of all kinds. If this were practical in terms of the operation of the Association, it would mean that those who neither hold office nor serve on APA committees could attend the meetings for a shorter time. This would possibly eliminate the problem of diminishing attendance during the last part of the week.

I would like to repeat that I am convinced that the APA convention does serve a worth-while purpose for the younger psychologists who are able to attend. I do believe, however, that a general tightening of the program would make it a more valuable experience.

DOROTHY SNYDER  
*University of Minnesota*

## SOME REMARKS OF A DUTCH VISITOR

To a visitor from abroad, and especially from a small country like The Netherlands, many things and events in America are "very" in size and amount, and so is an APA annual meeting. It is, in fact, nearly incomparable to what goes on at the annual conference of the Netherlands Institute of Practising Psychologists, which boasts of its 160 members (candidate members included). Such a conference is well attended if about 60 members find themselves together, and two sheets of paper suffice to contain the program for the two days.

So, when I gratefully accepted the invitation to attend the APA meeting—at that time I happened to be in the United States on a six-month study trip—I really did not have an adequate picture of what was going to occur. I presumed that it would be somewhat like an International Congress in Europe, with say 600 attendants, of whom a hundred might be active participants. But even this expectation turned out to be completely inadequate as soon as I received the July issue of the *American Psychologist* with its 200 pages of preparatory information. About 400 papers and 80 symposia, discussion groups, etc., and, in addition to this, special addresses, business meetings, film showings, and social gatherings! To an outsider who does not know too much of the inner structure of the APA and does not belong to any grouping, it is a little bit overwhelming. I remember the inner embarrassment I felt while trying to select from the plentiful offer a suitable program for the week to come, so that I would not be near to a breakdown at the close of it.

I remember also that I asked myself this question: why this abundance of papers? It is still an open question to me. I have been told, however, that the annual meeting has as one of its values that it serves as a "talent show" and as an exhibition of the achievements of the various research centers. Besides, there seems to be some evidence that this abundance of production is related to characteristics of the American culture and its emphasis on quick performance.

Two features that struck me in this conference may be linked with the presentation of so many contributions, although a full explanation might call for a consideration of American life in general. First, I have the impression of less personal involvement (of the speakers as well as of the audi-

ence) than at a European conference. The atmosphere may well be illustrated by the words with which N. P. Cantor ended his original contribution to a symposium on teaching methods: "Take it or leave it, and God bless you." When one is flooded with material, it is impossible to be deeply concerned with every single thing. It seems to me, however, that the apparent overloading of the program does not sufficiently account for the equally apparent unconcernedness; it must be rooted in something else. A second characteristic, undoubtedly related with the first, was the awareness of so many contributors of the relative value of their findings, opinions, and views. In Europe, still too many like to consider their personal standpoint as something of absolute value. Although apostolic fire has its charm, even in science, the general admission of the bounds of our cognitive power, as manifested at the conference, was a refreshing experience. This attitude was perhaps also one of the main factors in creating an atmosphere of tolerance and willingness in so many discussion groups; a releasing atmosphere of "don't take yourself too seriously."

Maybe these two features show the two sides of one medal, and maybe the unacceptableness of the reverse can be raised only when the awareness of the relativity of personal opinions stimulates a deeply concerned search for what is of real, general, and absolute value in science.

By far most interesting to me were some of the symposia and discussion groups I chose to attend, most of them in Divisions 2, Teaching of Psychology, and 8, Personality and Social Psychology. They offered an opportunity to get somewhat acquainted with the divergent views of American psychologists on several subjects of interest. Apart from this, it seems to me that any conference would gain by reducing the number of papers read to an audience, listening or not, and by increasing the number of discussion and round-table groups. And again the question arises, why these 400 papers? It is my conviction that we cannot dismiss all the lectures for our students—we need them for a condensed transfer of knowledge—but, this argument does not hold for an APA meeting.

One of the most enjoyable and successful discussions among those I attended was that on "Needed Research in the Teaching of Psychology"

(C. R. Carpenter, chairman). Out of an initial chaos of contributions and opinions gradually grew some clear-cut ideas about the factors involved in the teaching procedure and about the problems that research will have to face. In addition to this, I would like to mention the symposium on "The Effects on Segregation" (Gerhart Saenger, chairman), which was encouraging, not primarily because of its formal characteristics, but because of the attitudes displayed in the discussion. In European eyes, race discrimination and segregation are weak spots in the American democracy (in spite of a certain understanding of the historical background of the phenomena), so it was a pleasant experience to discover that the APA perceives the problem in a way which is not distorted by prejudices derived from the past.

One other aspect remains: the topics representing the mainstream in American psychology today. Surprising, at least at first sight, was the strong position of the more or less classic experimental psychology, which lost so much ground in Europe, especially after the collapse of scientific psychology in Germany in the early thirties. America is apparently the only country which has sufficient resources to maintain this theoretical research on a larger scale. Maybe other factors, such as the still prevailing interest in quantification and the continuous development of statistical devices, have their influence. More important to me, however, seems to be the fact that American experimental psychology has not lost its way in the maze of *possible* topics for research, but has developed in close contact with the needs of the reality of social life: it concentrates on problems

of perception, learning, and motivation. Thus, it fits in nicely with America's main concern, the problems created by lack of adjustment and of accomplishment and by deficiencies in human relationships, and the efforts of dealing with them along different lines. All this was well represented at the conference, and in great variety.

On the other hand, I did not find too many contributions dealing with the fundamental problems of psychological science, e.g., problems related to basic conceptions and assumptions, problems of definition and terminology, epistemological problems in practical methodology, problems of values and norms. Does the absence of contributions in these fields mean that there is no divergence of opinion among American psychologists and, consequently, no reason to discuss these problems? Or does it mean a trend toward emphasis on action in experiment, research, and practical problem solving rather than on reflection on basic principles and issues? Perhaps it is something else. I would like to propound it to the APA as an open question that deserves attention.

To conclude these remarks, I would like to say that this APA convention fulfilled a double function for me. I not only got a little bit acquainted with present-day psychology on this continent, but I also met colleagues and made contacts which I hope will be of value to the scientific development and the training of students in my own country. Moreover, the exhibitions of books, equipment, etc. helped to bring me up to date. Thus, the conference was in many respects a worth-while experience.

TONKO T. TEN HAVE  
University of Amsterdam

## Comment

### "Neo-Freudian" or "Neo-Adlerian"?

#### Report on a Survey Conducted among Members of the American Psychoanalytic Association<sup>1</sup>

**The problem.** In recent years the term "neo-Freudian" has been used with increasing frequency to refer to deviators from Freud whose position may be briefly described as stressing social relations rather than biological factors, the self rather than the id and the superego, the striving for self-actualization rather than the sex instinct, and the present situation rather than early experiences. On occasion, however, it has been pointed out that these new theories are actually a restatement in somewhat different terms and a further development of theories which were originally formulated by Adler. Wittels<sup>2</sup> suggested as early as 1939 that "neo-Adlerian" would be the more adequate designation of these deviations. We concur with Wittels on this, although his evaluation of Adler and our own are opposed, Wittels being a classical Freudian and we in sympathy with Adler and the above-mentioned current developments. If these are in fact closer to Adler than to Freud, then the current usage of the term "neo-Freudian" does injustice to Freud as well as to Adler and, beyond representing a personal injustice, obscures psychological theorizing. In order to obtain a judgment on this issue, we decided to submit the question "neo-Freudian" or "neo-Adlerian" to the members of the American Psychoanalytic Association because they would understand Freud and the deviations from him better than any other group, and would, as a matter of Freudian tradition, also be keenly aware of Adler.

**Procedure.** Between April 19 and 25, 1952, a self-addressed and stamped post card with the following two questions was mailed to the 444 members of the American Psychoanalytic Association: (1) "Do you consider 'neo-Freudian' the more CORRECT designation for the movement in psychoanalysis which today goes by this name, or would 'neo-Adlerian' be more CORRECT?" (2) "Regarding psychoanalytic theory, do you consider yourself a Freudian or rather a neo-Freudian?" The second question was asked because among the members some are deviationists, and we assumed that these would be more favorably disposed toward the term "neo-Freudian" since they benefit by the prestige value attached to the name of Freud. For the first question the respondent could choose among the

alternatives: " 'neo-Freudian' is more correct," " 'neo-Adlerian' would be more correct," and "neither is correct." For the second question the choice was among "Freudian," "Freudian with reservations," "neo-Freudian with reservations," "neo-Freudian," and "none of the above." The post card questionnaire was accompanied by a letter which explained the study and named as representative "neo-Freudians" Erich Fromm, Frieda Fromm-Reichmann, Karen Horney, Abram Kardiner, Patrick Mullahy, Harry Stack Sullivan, and Clara Thompson.

**Results.** The issue covered by the survey was apparently of lively interest to the psychoanalysts. Fifty-one per cent, that is 226 psychoanalysts, replied, and among these, two-thirds signed their names to receive a copy of the results, while one-fourth spontaneously made comments which in seven cases took the form of lengthy letters. The quantitative results from the two questions submitted are as follows:

Choice of Name	Theoretical Orientation of Respondent					
	Classical Freudian		Neither, or No Answer		Freudian-with-Reservations	
	N	%	N	%	N	%
"Neo-Adlerian"	59	41	1	3	6	11
Neither, or no answer	75	53	29	97	20	37
"Neo-Freudian"	8	6	0	0	28	52
Total	142	63	30	13	54	24

The largest category, the classical Freudians, comprising 63 per cent of all respondents, preferred "neo-Adlerian" to "neo-Freudian" at a 7 to 1 ratio (41 per cent against 6 per cent) for those who made a choice between the two terms, while 53 per cent considered neither term correct. If we regard the term "neo-Freudian" by itself, it was rejected by 94 per cent of this group. Secondly, the 24 per cent of the respondents who considered themselves Freudians-with-reservations, including 6 "neo-Freudians"-with-reservations and 4 "neo-Freudians," in the majority favored the term "neo-Freudian," preferring it to "neo-Adlerian" at a 5 to 1 ratio. This was as we expected for the reason given above. Finally, the 13 per cent who refused to commit themselves theoretically also refused to accept either of the two suggested names.

**Representativeness of the results.** This was tested by analyzing the replies according to time of arrival.

<sup>1</sup> This survey was supported by a grant from the University of Vermont.

<sup>2</sup> WITTELS, F. The neo-Adlerians. *Amer. J. Sociol.*, 1939, 45, 433-445.

Replies received during the first 9 days (70 per cent) were compared with those received during the following several months (30 per cent). The most important outcome of this analysis is that for the classical Freudians the ratio of "neo-Adlerian" to "neo-Freudian" remained the same, namely 43 to 6 for the 98 early, and 16 to 2 for the 44 late respondents, while the "neither" choices increased from 50 to 59 per cent. We therefore consider it reasonable to assume that for the nonresponding classical Freudians the percentage of "neither" choices, in continuation of the observed trend, would be still greater, while the proportional preference of "neo-Adlerian" over "neo-Freudian" would remain equally great.

#### IMPLICATIONS FOR THE PSYCHOLOGIST

Regarding nomenclature, the use of the term "neo-Freudian" should be seriously questioned, since only 6 per cent of the classical Freudian respondents consider it correct. "Neo-Adlerian" might be considered instead, since 41 per cent of these respondents judge it to be more correct. Or else, since over half of all the respondents approve of neither term, a new generic term might be desirable. In this connection, however, we found that in spite of numerous spontaneous comments, none of the respondents came forward with the suggestion of a new term.

Aside from the problem of terminology, the considerable endorsement of the term "neo-Adlerian" for the theories which presently go by the name "neo-Freudian" must be interpreted as a judgment by the classical Freudians that these ideas which today find increasingly wide acceptance are essentially those which Adler originally expressed, although they are generally not recognized as his. The implication of this judgment for the psychologist is to re-examine Adler for his relationship to these new developments, in the interest of a truer historic perspective and better systematization and clarification of personality theory.

H. L. ANSBACHER  
University of Vermont

#### Comments on the Proposal for a Psychological Museum<sup>1</sup>

##### PSYCHOLOGICAL MUSEUM

Historiophile wants a museum of old historically significant psychological apparatus in the new APA building. It would be a splendid thing to have if it could be properly arranged and cared for. It would be interesting and it would dignify psychologists by linking them up to a respectable, if quaint, pre-electronic ex-

<sup>1</sup> See *HISTOROPHILE*. A psychological museum. *Amer. Psychologist*, 1952, 7, 596.

perimental past. And I can speak of this matter because in 1915 I made the second inventory of the Cornell Laboratory (the first was the printed one of 1900) and then in 1920 the first inventory of the Clark Laboratory. I never inventoried the Harvard Laboratory; E. B. Holt had done that. So let us dream now about an apparatus museum just to see what is involved; the project may not turn out to be as simple as it sounds.

There would have to be a responsible chap who is keen about this museum idea, so keen that he will put in more hours than he is paid for, a man who is fascinated by recreating first for himself and later for others the atmosphere in which the experimental psychologist used to work. He had better be youngish and a PhD from a university where German and French are not yet extinct. He will have more work in building up the museum than in running it later, so he might start as a full-time man, or else as a half-time man for twice as many years. We need not try to settle hours and salary here. We shall call this chap the Curator. Conceivably he might be a Curatrix, if you could find a woman psychologist more interested in motors than motives.

The Curator will begin by getting up a preliminary sophistication about psychological apparatus. He will go to the manuals (e.g., Wundt, Hering, Titchener) and the handbooks and the old apparatus catalogs (Zimmerman, Spindler and Hoyer, Max Kohl, Verdin, and even Stoelting). He will write to our Life Members and get them to reminisce about experimentation in the 1890's and 1900's and what there was around the labs in those days that was already old and tradition-haunted. He will begin to get a line on where the desirable pieces are, and he will write to directors and ex-directors for more explicit information about what is gathering dust in the attics of the old laboratories. Then he will try to cadge the best thitherto neglected pieces from the old laboratories. That will be difficult, for forgotten dusty pieces at last wanted will suddenly seem valuable, and directors will reflect that in any case they have no right to give away what is the property of trustees or regents. So perhaps the frustrated Curator presently commits suicide and the museum space gets filled with back numbers of journals. Let us suppose, however, that he succeeds in getting the nucleus of a museum. The rest could be attracted to this core in years to come.

The next job is display—space, protective glass cases, enough illumination. And probably then more space. The first estimates are almost sure to be too meager. You must not just jam stuff in together. Things ought to be set up as if they were ready to work and not stood up as hunks of machinery. A Hipp chronoscope, with all its appurtenances (control hammer, reaction keys, commutators, calibrating chronograph), set out as

if ready to operate, would take up considerable space. Nothing else should stand so near as to appear as if it might be a part of the Hipp set-up. Broken parts of apparatus should be mended and missing parts replaced, but with clear indication of what is new and what is original. There should be labels and descriptions of such degree of elaboration that the visitor can understand, without consulting his catalog, what the apparatus and its special parts are for. Particular parts would need their special labels.

There should, however, be a guide or catalog too. Getting it up is the way the Curator has fun, and the result could be just ever so good. It would describe each piece and how it works, date it, make some effort to recreate the atmosphere of the time when research or demonstration required that kind of apparatus. It might mention how improved technics made the piece obsolescent (fall phonometer outmoded by electronics). In the early years the catalog would be steadily growing and could consist of mimeographed sheets stapled together from time to time.

If anecdotal material turns up, it could be included in the catalog. In any case for the Galton whistle you would wish to tell how it came to be miscalibrated so that psychologists thought for years that the upper limit of hearing was somewhere in the region of 30,000 to 50,000 cps. What about Appunn père? Did he really die of a broken heart when his tuning forks for the high frequencies turned out to be in error, as Titchener used to say? And then there is the American who wondered why Wundt was forever calling the Hipp chronoscope pretty, mishearing *das Hipp'sche Chronoskop* as *das hübsche Chronoskop*. I can contribute the rubber stamp for marking coordinates on the skin when temperature spots are to be mapped, the one that, lost in the post office at Drake University, was used for years to cancel mail. The Curator would appeal to the Life Members for this anecdotal material; they ought to earn their keep.

A museum that works is much better than a dead one. This is another matter that the Curator should keep in mind. Suppose that he could get from Cornell or Harvard or Clark an Ellis Harmonical, built to illustrate Helmholtz's *Tonempfindungen*. It has in it many of the enharmonic intervals. How many persons have ever heard a comma (about  $\frac{1}{6}$  semitone, sounds more like a mistuning than a change of note), and how many know the difference between a major tone (204 cents) and a minor tone (182 cents)? The Ellis has these and all the first 16 harmonics of 132 cps. What fun if a vacuum could be permanently put on this organ and the visitor given enough keys outside the case so that he could hear the different intervals and the different harmonic patterns under his own control. Would there be

any chance of getting a Helmholtz clang synthetizer ("vowel apparatus")? It might be set up to work, not with its own driving fork in which the spark would oxidize, but with some modern electronic driver at 128 cps, with the visitor put in command to give himself harmonic patterns and, following the instruction card, what Helmholtz thought were different vowels. And so on and on.

Another way to make a museum less dead is to bring into focus special pieces at different times for temporary veneration. There could be a shrine where something special with extra information could show itself for a time, being then replaced by something else. And pieces on loan and not left permanently with the APA could be displayed there. The frequent visitor to APA would gravitate toward this shrine to see whether the focal exhibit had been changed.

It is not impossible that an historical museum of this sort would develop eventually into a science museum for psychology, where the visitor with modern technics could take his own reaction time, see polaroid stereoscopy, pulling his binocular fusions out into diplopia, determine for himself various visual and auditory thresholds, and use Galton's shot apparatus to throw himself a normal distribution. There are any number of good perceptual visual demonstrations, from Ewald Hering to Adelbert Ames. Does this sound ambitious? Why should not American psychology be ambitious and bold in its undertakings? Look at what it has achieved in its first fifty years and in its most recent ten!

EDWIN G. BORING  
Harvard University

#### IN FAVOR OF A MUSEUM

I am shocked by the feeling tone aroused in me as I block out this note prompted by "Historophile." Introspection, if one may use that technique these days, reveals that I am rapidly becoming an oldster because my first explorations in the field of psychology go back more than thirty-five years.

Seeing, as I do, many psychologists with the bloom of youth on their cheeks and the vigor of fresh knowledge backing up their drive, I am constantly amazed by their lack of appreciation of what has gone on before their Don Quixote enknightment.

I am all for this selective accumulation of psychologicama, for truly rich sources of interesting material still remain. Today's psychologists and those yet to come will deeply appreciate seeing this rich foundation of our important field of study.

L. N. YEPSEN  
State of New Jersey  
Department of Institution  
and Agencies

## ACQUISITIONS FOR THE MUSEUM

I am enthusiastic over Historophile's suggestion that we establish a psychological museum. I, too, know of historical treasures that take up much-wanted room in various laboratories.

For example, I was informed that the correlation-calculating machine devised by Clark Hull in the 1920's is gathering dust in an obscure storage space at the University of Wisconsin. At the University of Mexico I saw a roomful of dust-covered apparatus which J. Mark Baldwin had installed when he became professor there in 1915. Perhaps the University would be willing to donate the entire lot. At the University of Wyoming I once saw a collection of autographs of the leading psychologists of the former generation, assembled by the late June Downey. C. H. Stoelting might have no-longer-marketable pieces which they would donate.

If you should test the feasibility of the idea by requesting the executive officers of the older laboratories to send in lists of materials they could contribute, I feel sure you would receive an encouraging response. As usual, in enterprises of this nature, the assembling of materials should start now while persons are living who belong to the second generation of American psychologists and who are able to document articles whose history and significance they alone know.

I can foresee that in the year 1992 when the APA celebrates its centennial in Washington, the proposed museum might be the most attractive feature of the meeting.

HARRY D. KITSON  
New York City

## JOURNAL OF THE HISTORY OF PSYCHOLOGY

I am one of the readers sharing the views of "Historophile" in connection with the proposal regarding a psychological museum. Furthermore, it seems to me that consideration should be given also to the possibility of establishing a journal devoted to the history of psychology.

In the field of medicine, there are at present two historical journals, *The Bulletin of the History of Medicine* and *The Journal of the History of Medicine and Allied Sciences*. Although work in fields related to medicine is published in these journals, I believe that psychology is barely represented, if at all. Allowing for differences in the history and development of medicine and psychology, it appears to me, nevertheless, that a sufficient number of qualified persons would be interested in and would contribute pertinent studies to such a journal of the history of psychology.

JEROME M. SCHNECK  
New York City

## Lecture Notes—1890

After the death of Professor Josiah Royce of Harvard, I was able to purchase a number of volumes from his personal library. Among them was the 1887 edition of George T. Ladd's *Elements of Physiological Psychology*. On the blank pages in front of the title page are the following notes in Professor Royce's handwriting, which I think are not without some interest for psychology as it was in the early days.

## INTRODUCTORY LECTURE ON PSYCHOLOGY

- I. Object of Ps.: The study of the Mind.
  - a. General purpose of scient. study, & nature of man's interest in his fellows.
  - b. Early conceptions of mind.
  - c. Growth of physiolog. conceptions.
  - d. Problem of Physiolog. Ps.
    1. Empirical.
    2. Philosophical.
- II. Mind: How known.
  - a. Difficulty of introspection.
  - b. Reason of difficulty in the social character of mental life.
  - c. Concrete introspection exemplified.
  - d. Sources of empir. knowl. of mind:
    1. Concrete introspection.
    2. Study of "human nature"; novels, etc.
    3. Physiolog. Ps.
    4. Abnormal mental life.
    5. Children.
    6. Sociological study.
    7. Comparative Psychol.
- III. Definitions.
  - a. Mind and Mental Life.
  - b. Consciousness.
  - c. Sensation, Perception, Feeling, Intellect, Volition, as categories of mental facts. The "faculties."

I believe that the outline of this introductory lecture can be placed in time. On the opposite blank page appears the following:

Course of '89-'90.  
Jan. Work. P. I, Ch. V, Par. 1-3, and 8-22.  
P. II, Ch. III, Par. 1-7, and  
Ch. IV, the whole  
Ch. VI, Par. 1-16, 21-26, 28-30  
Ch. VII, Par. 1-9, 13 ad fin.

The material of these references includes primarily the discussion of the sense organs, and the general discussion of the sensations experienced, qualitative, quantitative, spatial, and temporal.

SAMUEL W. FERNBERGER  
University of Pennsylvania

## Across the Secretary's Desk

### Relations with Psychiatry

In late February a bill which would have brought psychotherapy completely into the legal domain of medical practice was introduced into the New York State legislature. The bill, designed as a modification of the New York medical practices act, would have defined medicine as the "diagnosis and treatment of all physical and mental conditions."

The bill was apparently aimed directly at clinical psychologists but it is clear that the inclusion of "mental conditions" in a medical practices act means severe restrictions on several other professions which now employ psychological techniques in serving human individuals. Such an inclusion also clearly means great difficulty for any court of law called upon to define, in the process of trying an individual accused of practicing medicine without a license, such terms as "treatment" and "mental condition."

The bill was introduced into the legislature without any prior knowledge on the part of psychologists. When they learned of the bill, New York psychologists, working through the Joint Council of New York State Psychologists on Legislation, protested vigorously. At one meeting, on March 3, Dr. Rollo May, Chairman of the Joint Council, spoke as follows: (*New York Times*, March 4, 1953)

"This bill, if passed," he said 'would set the clock back one hundred years. . . . What is needed . . . is that people be helped to learn to live together harmoniously. If we simply continue emphasizing that man is a physical machine, we produce only more effective soldiers, and our civilization will indeed be threatened.

"The greatest harm in this bill is precisely that it makes this emphasis in stating that emotional and mental conditions are subordinate to medicine."

In a separate memorandum, Dr. May expressed these four points as representing the views of 2,200 psychologists in this state:

1. 'Mental conditions' is a term that is impossible to define. A great part of the work of teachers, social workers and clergymen as well as psychologists deals with 'mental conditions,' as do in-

telligence tests, vocational tests, guidance and so on.

2. The bill is administratively unfeasible since the definition encroaches on so many fields.

3. The bill is an 'overstepping of the prerogatives' of medicine. Since Governor Dewey vetoed a bill in 1950 to license psychologists on the basis that the distinction between psychology and psychiatry could not be adequately defined, 'it is obviously inconsistent and illogical' that an amendment now offered proposes to define the mental field as entirely a branch of medicine.

4. A commission should be set up to study 'licensing in the healing arts.'

The American Psychiatric Association responded the next day through Dr. Wilfred Bloomberg, chairman of the Association's Committee on Public Education. The *New York Herald Tribune* of March 5 quoted Dr. Bloomberg as follows:

"The practice of psychotherapy is a province of medical practice. The training of other professional groups does not qualify them for the independent practice of psychotherapy or any other kind of medical treatment.

"In view of attempts to establish the practice of psychotherapy independent of medical practice, by various proposed laws which re-define the area of responsibilities in the field of health, we must reaffirm that these attempts to separate psychotherapy from the practice of medicine and from medical responsibility are contrary to good public policy.

The American Psychiatric Association calls attention of its members to the fact that a physician who takes responsibility for the treatment of a patient assumes full responsibility, and, while he may utilize the services of other disciplines, he cannot relinquish to a non-physician his medical responsibility."

These New York developments were reported immediately by telegraph to the American Psychological Association Board of Directors who, on March 3, unanimously passed the following motion:

"Moved that Board of Directors of APA is opposed to legislation restricting to any one profession the application of psychological techniques and knowledge. Public welfare demands that such services be the joint responsibility of many profes-

sions, including psychology, medicine, education, the ministry, and social work, and should not be limited to any one of them."

On March 7 the *New York Herald Tribune* carried an editorial on the legislative situation. It said in part:

"The psychologists, especially, have a case. They asked the Legislature two years ago to license them, and they offered a supposedly model bill which had the enthusiastic support of the State Department of Education. The bill was enacted by both houses, but was vetoed by Governor Dewey because of what he deemed to be a serious conflict of opinion between the psychologists and the medically trained psychiatrists over the legitimate field of activity for psychologists.

"There are almost five thousand (sic) highly trained psychologists in this state, but there are also about twenty-five thousand quacks at large. The bill introduced in the present session by Sen. Wheeler Milmoe, R., of Canastota, would amend the educational law to require general medical training for persons who treat patients suffering from any 'mental condition.' This is an extremely exclusive amendment as far as psychology is concerned. It purports to crack down on the quacks, but it would restrict competently trained psychologists from treating individuals for mental disturbances. Conceivably it also could be extended to restrict clergymen, social workers and others whose normal activities are helpful in rehabilitating maladjusted individuals. The amendment is much too sweeping, and it should not be enacted.

"Is it not of significance to the Legislature that the psychologists and the medical profession, though at odds on the Milmoe bill, both favor the Metcalf proposal for a careful study of the whole situation? Does the Legislature continue to be turned against the Metcalf bill by the serious charge that it is designed only to block the chiropractic licensing measure? These proposals involve matters vital to the public, and the Legislature would do well to have them all studied before acting on them."

The bill died in Committee. In New York, then, a situation of intricate and potentially bitter inter-professional conflict has been temporarily averted. But the general problem giving rise to the New York State situation is by no means solved.

The *Journal of the American Medical Association* for December 27, 1952, published a report of

the AMA Committee on Mental Health that said, in part:

"The Committee further recognizes that clinical psychologists may have need for standardization of their qualifications within their own field of endeavor for the purpose of preventing encroachment by those having insufficient education and training who now offer their services to the public. However, it is the impression of the Committee on Mental Health that the primary motivation of the clinical psychologists is to become recognized by law as qualified to enter the private practice of psychiatry under the guise of psychotherapists. Since the Committee is convinced that clinical psychologists cannot in any way be qualified by their training and experience to function independently as psychotherapists, it seems necessary that the American Medical Association adhere to a policy which defines the practice of psychotherapy as an integral part of the practice of medicine and therefore present a medical point of view in relation to any attempt at legal recognition of clinical psychologists. . . . The Committee on Mental Health reaffirms that the practice of psychotherapy is but one aspect of the total therapeutic armamentarium of a physician and therefore cannot be split off as an isolated technical skill divorced from the overall diagnostic and treatment procedures inherent in the practice of medicine. This affirmation in no way contradicts the recognition of the fact that other professional groups, such as ministers, lawyers, teachers, social workers, nurses, psychologists, vocational counselors, and other professional groups, now utilize psychological understanding and principles in the carrying out of their particular function. . . . It is deemed advisable that all medical practice acts include the treatment of illness by psychological methods. It is further recommended that the American Medical Association take active steps toward this end.

"The committee further recommends that the suggestions of the Gerty report relating to all present and future proposals for the certification or licensure of psychologists be carefully studied to determine whether they are in accord with the principles and recommendations here expressed.

"In view of the fact that the policy of the American Medical Association has always emphasized the best interest of the public welfare, it should be a matter of record that the Association stands pre-

pared to collaborate with all recognized professional groups in maintaining the highest standards."

This statement came as a surprise to our Committee on Relations with Psychiatry and as an apparent surprise to some members of the American Psychiatric Association's Committee on Clinical Psychology. At the time the statement was issued the two committees were doing their rational best to confront interprofessional problems constructively. There is some evidence that the AMA stand is embarrassing to many psychiatrists who are convinced that psychiatry and psychology can and must work in mature, amicable, and nonhierarchical collaboration if public welfare is to be served in the field of mental health.

That the AMA stand is by no means embarrassing to all psychiatrists, however, is illustrated by the February 1953 newsletter of the American Psychiatric Association stating that the Executive Committee of the Association "went on record that medical practice acts in the states should include a statement that the practice of medicine includes the diagnosis and treatment of all mental and physical ills."

There does not appear to be, at the moment, a concerted campaign on the part of either the AMA or the American Psychiatric Association to implement the recommendation that "mental conditions" or "mental ills" be written into medical practice acts. But the official positions are very clear. It seems a good possibility that there will be attempts in many states to include mental ills or mental conditions under medical hegemony.

What do psychologists think about the inclusion of mental conditions or mental ills in the medical practices acts? We have been deliberately slow in taking official stands on such a question. We have certain inhibitions about taking official action on anything in the absence of sound evidence, full discussion, and reasonable consensus among concerned members. There have been, however, two official actions of direct relevance for our relations with the medical profession. There is the March 3 motion of the Board of Directors. And there is the following motion, passed by the Council of Representatives in 1949:

"We are opposed to the practice of psychotherapy (not to include remedial teaching, vocational and educational counseling) by clinical psychologists that does not meet conditions of genuine collaboration with physicians most qualified to deal with the

borderline problems which occur (e.g., differential diagnosis, intercurrent organic disease, psychosomatic problems, etc.)."

These statements make it clear that psychologists join with physicians in believing that psychotherapy should not be entirely divorced from somatic diagnosis and treatment. Psychologists also agree that psychological training does not qualify an individual to practice medicine, as presently defined. Psychologists will not agree that psychotherapy is a subordinate part of the practice of medicine. We agree that psychologists must concern themselves with standards of competence and of ethics, and we have taken steps to insure that psychologists who engage in independent practice meet high standards: we have adopted a provisional code of ethics, we have established a specialty board (the American Board of Examiners in Professional Psychology), and we are working through our Education and Training Board and other mechanisms to establish and create high and meaningful standards of training for psychologists.

Since psychology has an enforceable code of ethics and since there are adequate laws relating to fraud and damages, psychologists see no basis for the feeling that the practice of clinical psychology by competent individuals will be detrimental to public welfare. Psychologists are not likely to take kindly to any real or apparent steps on the part of any other profession to police or supervise professional activities of psychologists. Making psychology ancillary to medicine in the field of mental health will be seen as no more reasonable than making the psychology professor an assistant to the professor of physiology or of making the professor of theology ancillary to the professor of psychology. Few will be able to see how the establishment of professional or scientific hierarchies will contribute either to public welfare or to the advancement of knowledge.

At the operating level there seem to be few professional or jurisdictional conflicts between clinical psychologists and psychiatrists. When confronted with an actual job, the psychologist and psychiatrist react to the job as a job and to each other as individuals. When professional labels are applied, however, and when professional organizations adopt "party lines," there is conflict. Official stands are taken, battle stations are manned, and the common enemy—emotional problems of humanity—is somehow forgotten. To the outside observer, a conflict

between psychologists and psychiatrists makes no sense. It makes such nonsense, in fact, that one observer has suggested that the impending conflict be settled by requiring that the two executive officers of the two APA's meet at dawn—repeatedly, if necessary—and fight the matter out, leaving practicing psychiatrists and psychologists free to go about their proper work.

The objective observer would probably find that both psychologists and psychiatrists sometimes over-reach their training. He would also find that, when over-reaching is demonstrated, the professional peers of the transgressor—whether the peers be psychiatrists or psychologists—will judge him strictly. Certainly it is true that psychiatrists are vitally interested in the competence and ethicality of practicing psychiatrists. It is equally true that psychologists, with their traditional involvement in problems of selection, training, and validity, are vitally and intricately interested in the problem of what characteristics, what training and what behavior is conducive to good professional performance. The profession of clinical psychology is still much younger than that of medicine, of course. But there is considerable evidence that psychology is maturing—maturing rapidly. It is setting up standards, interiorizing ethical values, policing itself, exploring for its best ways of serving human individuals. The present question seems to be that of whether or not this youthful profession shall develop in accordance with its own conscience and its own intelligence, or be brought along with other professions under the parental discipline of the much older medical profession.

There undoubtedly are a number of self-appointed "psychologists" operating in the mental health field. Legitimate psychologists join heartily with psychiatrists in seeking ways to see that charlatans do not harm or gull the public. To many psychologists the sensible way to prevent untrammelled charlatanism is through state licensure or certification of adequately trained psychologists. In some states, such controls have been established. In other states attempts to establish legislation of this sort have been blocked, most often by medical opposition.

It may be that the recent actions of the American Medical Association and of the American Psychiatric Association have closed the door to further factual exploration and rational discussion. At least on the surface it looks as if psychologists now

have only two alternatives: (a) to submit quietly to the medical profession or (b) to oppose openly and vigorously, in whatever ways they can, the move to establish medical hegemony over the field of mental health. To the thoughtful psychologist, neither alternative makes any sense.

To do nothing while "mental conditions" are made the exclusive concern of the physicians would represent to many not only an immorally weak submission to arbitrary authority but also compliance with a move that appears likely to decrease the amount of professional help available to those millions who have personal and emotional problems.

To wage a public fight, on the other hand, would be almost equally undesirable. It would be a sorry spectacle indeed to see two professional groups fighting, in the press, in the legislatures, and in the courts, about who has the right to help people with their psychological problems. Such a fight could set back both psychiatry and psychology perhaps more than any other development imaginable. Many observers agree that both psychiatrists and psychologists enjoy only an uneasy public acceptance, that there is a great deal of general underlying hostility to anyone who represents himself as knowing about the minds and motives of men. The current trend toward anti-intellectualism in the country at large makes it even more important that "mind doctors" commit themselves with great decency and maturity if they are to preserve their opportunity to practice their profession. A public argument, such as the one that began in New York, can do inestimable harm to both our professions and eventually to the public we attempt to serve.

Is the die irretrievably cast? Must psychologists and psychiatrists fight a fight that will, in the long run, harm both professions? Can these inter-professional problems again be opened to reasoned examination and reasonable discussion? We hope so. Thoughtful psychologists everywhere will probably want to discover ways to escape the dilemma onto which we have been thrown.

The informed and intelligent citizen, in viewing the whole problem, would probably conclude that no one is now in possession of sufficient evidence or sufficient wisdom to crystallize for all time the relations between psychiatrists and psychologists. The citizen would want hard evidence and reasoned logic upon which to base his conviction about how best to protect public welfare. He would have no respect for the "psychological viewpoint" or the

"medical viewpoint" per se, but would instead endorse the human viewpoint. We can best help whom with what sort of problem? How can the best possible help be given to the million or more Americans now in real need of some manner of psychotherapeutic assistance? What will be the implications for public welfare in twenty years if clinical psychology is made ancillary to medicine? Will it drastically reduce the number of professionally trained people available to the public? Will it increase the number of pseudo-professionals operating, in spite of laws, in the field of mental health? If there are not enough competent people to meet the public need, to whom will the public turn? What do clinical psychologists now do that is harmful to clients? What do they do that causes physicians concern? What professional people are now using what sort of psychotherapeutic techniques and with what results? How does anyone tell whether an individual is skilled and successful in applying psychological techniques? How many psychiatrists are skilled in psychotherapy as well as other approaches to mental disorders?

It still may be possible to reinsert the informed citizen—either through empathy or through more tangible means—into the present dispute. In the long run, it is his welfare that is at stake, for one out of ten of him will spend time in a mental institution and perhaps one out of five of him will have occasion to seek professional help with an emotional problem.

It should still be possible, both in national and local situations, to bring the objectively observing citizen back into the problem. If we could find ways to pool both the humanistic concerns and the intellectual resources of psychiatrists and psychologists, if we could bring ourselves to confront the perceptions of the informed citizen, we not only could obtain good evidence on the foregoing ques-

tions but could find joint and creative solutions—solutions demonstrably consistent with public welfare and with the genuine humanistic concerns of both psychiatrists and psychologists.

FILLMORE H. SANFORD

#### ADDENDUM

While the above material was on its way to the printer, the APA Central Office received word that Dr. D. Ewen Cameron, president of the American Psychiatric Association, had issued a public statement on March 17 that "the practice of psychotherapy is a province of medical practice." Dr. Cameron was articulating the sentiments of the Executive Committee of his Association. He goes on to say, "There is an ominous trend in proposed legislation in several states to 'license' persons other than physicians to treat mental conditions through psychotherapeutic techniques."

This statement received a limited coverage by the press. After thorough discussion among a number of psychologists in and out of Washington, the decision was reached that nothing could be gained by a precipitous reply at this time to Dr. Cameron's statement. It was the group feeling that psychologists, while standing ready to resist specific moves toward restrictive legislation, should make at least one further attempt to bring about rational discussion with psychiatrists on points of disagreement.

#### ADDENDUM II

On March 30 it appeared likely, as a result of recent conversations between the two APA's, that several officers of each Association would sit down soon, under a "cease fire" arrangement, and seek further for amicable and rational ways of settling the present points of disagreement between psychology and psychiatry.

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## *Psychological Notes and News*

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**Minnie L. Steckel**, personnel director at Alabama College, died on December 1, 1952.

**John L. Meadows**, dean of students at Tennessee Polytechnic Institute, died of a heart attack on January 8, 1953.

**Gustav Kafka**, professor of philosophy and psychology at Julius-Maximilians University in Wurzburg, died February 12, 1953.

**Clark University** has announced the following new appointments in its department of psychology: Richard A. Lazarus of The Johns Hopkins University, to be associate professor of psychology as of September 1, 1953; Tamara Dembo, as associate professor of psychology as of February 1, 1953; and Theodore Leventhal, chief psychologist of the Worcester Youth Guidance Center, as instructor in psychology as of February 1, 1953. Dr. Dembo will collaborate with Drs. John E. Bell and Heinz Werner of the department on research into the early psychological development of cerebral palsied children. This research is supported by a two-year grant from the Association for the Aid of Crippled Children.

The department of psychology at North Carolina State College announces the following appointments to its staff: Elva Burgess on a temporary appointment as an assistant professor replacing Clyde Johnson who is on leave of absence, Allen Solem and Charles Kelley as assistant professors. Dr. Burgess completed her degree at the Pennsylvania State College. Dr. Solem comes from the University of Michigan and Mr. Kelley from Dunlap and Associates.

**LeRoy H. Rook**, formerly at the Psychological Services Center of the University of Oklahoma, is now staff psychologist at the State of Missouri State Hospital No. 1, Fulton, Missouri.

**Dominick J. Lacovara**, Captain, M. C., has been ordered to active duty in the U. S. Army and is now assistant chief, psychiatry and neurology, 4002d, ASU, U.S.A.H., Camp Chaffee, Arkansas. He was formerly associate psychiatrist and chief psychologist at Craig House, Beacon, New York.

**Richard M. Rundquist**, formerly of the counseling center at Stephens College, has taken a position as assistant professor of education and counselor at the Guidance Bureau of the University of Kansas. He succeeds **Lee E. Isaacson** who has taken a position as assistant professor of education at Purdue University.

**Ernst G. Beier** has accepted the position of associate professor of psychology with the department of psychology of the University of Utah. His work will be identified with the clinical training program established at the University of Utah, with particular responsibility associated with clinical research and practicum supervision. Dr. Beier has been assistant professor of psychology and head of the mental hygiene services at Syracuse University.

**Albert K. Kurtz** is on leave of absence from Pennsylvania State College and has been appointed interim professor of psychology at the University of Florida for the current semester.

**Albert Egash and Irving Sigel**, formerly at Michigan State College, joined the staff of the Merrill-Palmer School, Detroit, in September 1952 as research psychologists. Dr. Egash is with the Division of Older Childhood and Adolescence. Dr. Sigel will direct a study of "Adult Influences Used in Controlling Child Behavior and their Psychological Effects on the Child." The study is sponsored by a grant from the Social Research Foundation.

**Ivan N. Mensh** was appointed associate professor and head of medical psychology in the department of neuropsychiatry, Washington University School of Medicine at St. Louis, effective February 1, 1953. Senior staff members in medical psychology are Bettye M. Caldwell and Joseph D. Matarazzo; fellows for 1952-53 are Alicemarie Meyer of University of Texas and Ross R. Thomas of Northwestern University; and research appointments are held by Goldine C. Gleser, Evelyn P. Mason, Frank B. Strange, and Ann L. Brockway.

**Jerome M. Schneck** has resigned as psychiatric consultant to the Westchester County Department

of Health to devote full time to private practice of psychiatry in New York City. He has retained his position on the psychiatric faculty of the College of Medicine, State University of New York.

**Robert G. Kaplan** is now associated with the Reese Graves Personnel Service in San Diego, California. Reese Graves Personnel Service engages in personnel and management consultations and services in the San Diego area.

The research department of the Bank Street College of Education announces that, with the addition of several new staff members, the research staff now includes: Barbara Biber, director; Dorothy Dinnerstein, Edna Kaufman, Claudia Lewis, and Harvey Schrier, research associates; Sheila Emerson and Virginia Stern as research assistants; and Lawrence Epstein, Evelyn Feltenstein, Reuben Margolis, and Marvin Zuckerman as part-time research assistants.

**Norma R. Metzner** has accepted an appointment at the APA Central Office to work on Project B of the APA contract with the National Science Foundation having to do with the evaluation of psychology as a science.

**Wesley S. Roeder** has been appointed guidance and evaluation consultant to the California Test Bureau.

The Director of the Institute for Advanced Study has appointed an advisory committee on psychology, in anticipation of the possibility that, from time to time, psychologists may desire to spend a year or more working at the Institute. The members of the advisory committee are: E. G. Boring, Jerome Bruner, H. S. Langfeld, Paul E. Meehl, George A. Miller, Edward C. Tolman, and Ruth S. Tolman.

#### *VA Department of Medicine and Surgery, Clinical Psychology Announcements*

**Benjamin Fabrikant**, a graduate of the VA Training Program from the University of Buffalo, has been appointed to the staff at VA Hospital, Buffalo, New York.

**Samuel Klugman**, a graduate of the VA Training Program from the University of Pennsylvania, has been appointed to the staff at VA Hospital, Coatesville, Pennsylvania.

**Robert G. Gibby**, formerly Chief Clinical Psychologist, VA Mental Hygiene Clinic, Detroit, is now Chief, Clinical Psychology Training Unit, VA Regional Office, Detroit, Michigan.

**Karl Pottharst** has been designated Chief Clinical Psychologist of the VA Mental Hygiene Clinic, Detroit, Michigan.

**Carolyn Pratt** has resigned from the staff at VA Hospital, Indianapolis, Indiana.

**Roy E. Buehler** has transferred from VA Regional Office, Huntington, West Virginia, to VA Hospital, Perry Point, Maryland.

**William Colley** has transferred from VA Hospital, Seattle, Washington, to VA Regional Office, Huntington, West Virginia.

**Stanford H. Simon** listed under VAH Knoxville, Iowa, in the January roster is on duty at VA Hospital, Indianapolis, Indiana.

**Martin J. Brennan** is Chief Clinical Psychologist at VA Hospital, Tuscaloosa, Alabama.

**Peter J. Napoli**, VA Hospital, Montrose, has been detailed to serve as Acting Chief, Vocational Counseling Service, to organize the service and supervise the training program.

#### *ABEPP Announcements*

The American Board of Examiners in Professional Psychology, Inc. announces the scheduling of its fifth written examinations for the Fall of 1953. Examinations will be given simultaneously at several examining centers on November 12-13, 1953.

Each eligible candidate will be notified individually and will be sent a registration form for this examination.

The Board wishes to mention again its policy concerning examination privileges, which was announced originally in the May 1951 issue of this journal. An eligible candidate holding the PhD degree who is notified regarding his eligibility for two successive written examinations and who does not present himself for either of these examinations will have his candidacy set aside.

Further, the Board wishes to call to the attention of interested members of the profession its policy concerning the amount of postdoctoral experience required for filing application for the Board's diploma. A total of five years of acceptable, qualifying experience is necessary. However, candidacies received up to and including July 1, 1953, need present only *two* years of such experience obtained beyond the award of the doctoral

degree. Three years, of the total of five, may have occurred previous to the award of the doctoral degree. Beginning July 2, 1953, the amount of post-doctoral experience required will be increased from two to three years. Professional psychologists who consider themselves to be eligible are invited to make inquiry and to file application. Candidacies may be processed in time for admission to the 1953 written examinations.

Correspondence concerning application and eligibility for examination should be addressed to Dr. Noble H. Kelley, Secretary-Treasurer, American Board of Examiners in Professional Psychology, Department of Psychology, Southern Illinois University, Carbondale, Illinois.

In September 1953 the psychology department of the University of Massachusetts will start offering graduate training toward the doctor of philosophy degree as well as toward the master's degree. Members of the staff are: Robert S. Feldman, William F. Field, Albert E. Goss, Bernard Mausner, Claude C. Neet, William W. Saunders, Aaron J. Spector, and Theodore R. Vallance (on leave of absence). For information concerning courses and other details write to Dr. Claude C. Neet, Department of Psychology, University of Massachusetts, Amherst, Massachusetts.

At the University of Michigan the departments of psychology and education have organized a joint, two-year program of training for "School Psychological Diagnosticians." The program includes a one-semester internship and is aimed to prepare students for giving and interpreting individual mental tests in schools, measurement bureaus, and psychological service centers, where the clientele is primarily children. It satisfies the requirements of the State Department of Public Instruction for approval for psychological work in the reimbursable program for the mentally handicapped.

#### *Announcements Regarding Graduate Training Facilities and Stipends*

The University of Denver offers the MA degree and the PhD degree in psychology in the following areas: general and experimental, clinical, counseling, and a joint PhD program with the School of Education in educational psychology. In the last three years 78 Master's degrees and 12 PhD degrees have been awarded. Four graduate

assistantships are available but are not open to new students. These pay \$600 each for the academic year and are continued through the summer quarter for students who choose to remain during that quarter. For information write to: Dr. L. W. Miller, Chairman, Department of Psychology, University of Denver, Denver 10, Colorado.

The University of Kansas offers graduate programs leading to both the MA and PhD in the following fields: educational psychology, counseling, and tests and measurements. For information write to J. H. Nelson, Dean, Graduate School, University of Kansas, Lawrence, Kansas.

University of Nebraska, Department of Educational Psychology and Measurements. MAT given as part of qualifying examination, but GRE accepted in its place. No scholarships. Four research assistantships; 10 hours' work; stipend, \$600 ex. Three teaching assistantships; half-time work; stipend, \$1,200. New students eligible. Apply by April 1 to Dr. D. A. Worcester, Chairman, Dept. of Educ. Psych. and Measurements. Available to graduate students in either department: Four assistantships (psychometry, research, counseling, reading and study laboratory); 15 hours' work; stipend, \$600-750 ex. Apply to Dr. Arthur A. Hitchcock, Director, Junior Division and Counseling Service.

The Post-Doctoral Institutes sponsored annually by APA Division 12 will be held this year from August 26 through September 2 at Kent State University, Kent, Ohio. Located thirty-five miles southeast of Cleveland, Kent State will provide appropriate classroom and lounge facilities and inexpensive accommodations for families as well as for individuals. Local transportation will make the move from Kent to Cleveland for the APA convention an easy matter. All members of Divisions 12 and 17 who hold the doctorate or who have been certified by ABEPP are eligible to attend. Enrollment for each course is limited to twelve. As in previous years, the tuition will be \$50.00.

Course offerings and instructors are as follows:

- Group Psychotherapy and Dynamics—Dr. S. R. Slavson
- Supervision in Integrative Psychotherapy—Dr. O. H. Mowrer (enrollment restricted to those who have taken prior didactic work with Dr. Mowrer)
- Learning Theory in Relation to Psychopathology and Psychotherapy—Dr. E. J. Shoben, Jr.
- Techniques and Methods in Clinical Research—Dr. Joseph Zubin

**Present-Day Problems in Psychological Therapy—Dr. Rollo May**

Applications for enrollment *must* be filed by June 1. Requests for application forms and further information should be addressed to Dr. Goldie Ruth Kaback, Chairman, Committee on Post-Doctoral Institutes, College of the City of New York, 139th St. and Convent Avenue, New York, New York.

The department of psychology, University of Chicago, announces two work shop seminars in the Rorschach test, July 6 to 10 inclusive, and July 13 to 17, 1953 inclusive, to be conducted by S. J. Beck. "Basic processes" will be the topic for the first week; "Indications for treatment," the second week. Scoring of test responses, significance of the response categories separately, their effects on one another in shaping the whole personality, technique and controls in obtaining a test record, and introduction to interpretation will be included in the basic processes seminar. In demonstrating indications for treatment, a series of cases will be presented in which the symptoms range from the ego-alien to the ego-syntonic. The questions for each patient, answers for which are sought in the test, are: at what level is this person to be treated? how much? what specific assets in him can be activated? The clinical pictures will vary from the milder personality disorders to questions of schizophrenia. Ages of patients will range from childhood into adult years. Admission to the advanced seminar is limited to psychologists and psychiatrists in clinical positions or practice. The basic processes may be taken by students at, or ready for, the intern level. Each seminar will meet at the University, Monday through Friday, two sessions each day, two hours each session. For information as to admission, fees, or credit arrangement when desired, write to the Executive Secretary, Department of Psychology, University of Chicago, Chicago 37, Illinois.

The Workshop in Projective Methods inaugurated in 1951 at the New School for Social Research, in New York, under the direction of Camilla Kemple and Florence R. Miale, will be held again this summer. There will be courses in introductory and advanced Rorschach, Figure Drawing Analysis, Sentence Completion, Handwriting Analysis; and an advanced Seminar in the Integration of Projective Methods. The staff will include, in addition

to the directors, Karen Machover, Elizabeth Anderson, James Q. Holsopple, Richard Benjamin, Ruth Lesser, and Irving Shulman. The dates are from June 8 through July 10 inclusive. The courses receive graduate credit from the New School and may be taken under the G. I. Bill. Inquiries may be addressed to Richard Benjamin, 34 West 75th Street, New York 23, New York.

O. Hobart Mowrer, research professor of psychology at the University of Illinois, will conduct a workshop on personality theory and psychotherapy from June 1 through June 5 in the Graduate Division of Psychology of Adelphi College, Garden City, Long Island. Admission will be limited to fifteen students and tuition fee will be \$50. Applications for admission, which will be limited to advanced doctoral students and practicing psychologists, should be addressed to Dr. Gordon F. Derner, Director, Clinical Psychology Training Program, Adelphi College, Garden City, Long Island.

The University of Minnesota Summer Session will present a Family Life Workshop the summer of 1953, through the cooperation of several graduate departments. The purpose of the workshop is to offer a synthesis of advanced practice at the graduate level for those now professionally engaged in family life education—teachers, marriage counselors, social workers, public health workers, research personnel and others in the family life field. The content of the workshop is being organized around four study areas—teaching, marriage counseling, research and evaluation, and community education and planning. The workshop dates are July 6-24 inclusive. The University offers four quarter credits for those who wish graduate credit. For further information write Mrs. Dorothy T. Dyer, Chairman, Family Life Program, 204 T.N.M., University of Minnesota, Minneapolis 14, Minnesota.

"The Counselor as a Person" will be the theme of the 1953 Chico State College Counseling Workshop to be held on the Chico State College Campus from June 15, through July 10, 1953. Among those who will present lectures and conduct discussion groups and classes are Donald E. Super, Clarence A. Mahler, Reverend C. Vin White, Hugh M. Bell, Margaret Wells, Merville C. Shaw, William Connor, and Arthur J. Manske. In addition to lectures and discussion groups, courses will be

offered in "Techniques of Interviewing," "Use of Tests in Personal Assessment," "Culture and Personality Growth," "Physiological Factors and Personality Development" and "Counselor's Personality and the Counseling Relationship." The Advanced Workshop will provide a laboratory for practice in counseling. The Basic Workshop will emphasize the courses given above. For information write to: Co-ordinator, Counseling Workshop, Chico State College, Chico, California.

Applications are being accepted for postdoctoral training fellowships in clinical psychology at the Menninger Foundation for the two-year period beginning July 1, 1953. The fellowship in general clinical psychology is designed to give competent clinical psychologists an opportunity to receive intensive advanced training in diagnostic testing, psychotherapy, research and child psychology, in order that they might accelerate their development into highly competent clinicians. The fellowship in clinical psychological research is offered tentatively, pending confirmation of support for this program. The training will have as its objective the development of those assets which would contribute to the research leadership potential of the Fellow as well as to his competence as a skillful and imaginative clinical researcher. Those applicants will be favored whose interests lie in teaching and research, and who may be expected eventually to pass on to new trainees some of the benefits they derive from this training experience. The program is supported in large part by the United States Public Health Service. Stipends totaling \$4,100 will be provided in the first year, and \$4,300 the second year. A PhD in psychology with a minimum of one year of supervised clinical experience is required. For information or application forms, write Martin Mayman, Director of Psychological Training, Menninger Foundation, Topeka, Kansas. Applications should be submitted no later than May 1.

A new publication, the *Journal of Medical Psychology*, is being considered, with first publication as the January 1954 issue. Medical psychology, a relatively new and rapidly expanding field, is considered to include relationships of psychology to psychosomatic and internal medicine, medical education, surgery, physical medicine, gerontology, pediatrics, and other medical specialties, as well as

to psychiatry and neurology. Research reports will be stressed but articles concerned with educational matters and the functioning of psychologists in new or different settings will also be included. Reactions to this proposal will be appreciated by the editor, Robert I. Watson, of Northwestern University.

The *Proceedings of the Fourth International Congress on Mental Health*, held in Mexico City, December 1951, have now been published in English and Spanish. The English edition is now available from the Columbia University Press, 2960 Broadway, New York 27, N. Y., for \$5.00 per copy.

Publication of the *Proceedings of the Second Research Conference on Psychosurgery* has been announced by the National Institute of Mental Health, Public Health Service, Federal Security Agency. Single copies (PHS Publication No. 156) are available free to professional personnel by addressing the National Institute of Mental Health, Bethesda 14, Maryland.

The Institute for Research in Human Relations, 2224 Locust Street, Philadelphia 3, Pa., would like to purchase one copy of each of the volumes listed below of the *Army Air Forces Aviation Psychology Program Research Reports*: Volumes 1, 2, 5, 6, 7, 8, 9, 10, 11, 12, 14, 15.

Psychologists doing research on stress and the adaptive hormones have been asked to send reprints of their articles, for use in the preparation of the Annual Reports on Stress, to Dr. Hans Selye, Institute of Experimental Medicine and Surgery, Université de Montréal, Montréal, Canada.

The Western Psychological Association will hold its thirty-third annual meeting at the University of Washington, Seattle, on June 18-20, 1953. Members of the Association at the University, under the chairmanship of Allan Katcher, are acting as committee on local arrangements. Officers for the current year are: Ruth S. Tolman, president; Neil Warren, vice-president; Richard Kilby, secretary; Rheem Jarrett, treasurer.

New officers of the Ontario Psychological Association are: G. H. Turner, president; H. O. Steer, past president; R. W. Ross, vice-president; E. T. Alderdice, secretary-treasurer; J. M. Blackburn, F. S. Dunlop, and E. M. Stapleford, directors.

The present officers of the American Association on Mental Deficiency are: Bertha M. Luckey, president; Arthur T. Hopwood, president-elect; Neil A. Dayton, secretary-treasurer; Lloyd N. Yepson, William W. Fox, Chris J. DeProspo, Herman Yannet, Thomas L. McCulloch, and Frances M. Coakley, vice-presidents. Two categories of membership are in effect, Associate and Active. Current fee is \$6.00 per annum for Associate Membership; one year's experience in field is necessary. The fee for Active Membership is \$8.00 per annum; three years of experience necessary. The fee includes subscription for the *American Journal of Mental Deficiency*, the A.A.M.D. *News*, and the Official Directory of Members. Applications and further information may be had from Neil A. Dayton, M.D., American Association on Mental Deficiency, P. O. Box 96, Willimantic, Connecticut.

The Gerontological Society will hold its annual meeting in the Mark Hopkins Hotel, San Francisco, on August 25, 26, and 27, 1953. Harold E. Jones is in charge of local arrangements for the convention.

#### CURRENT RESEARCH

The research program of the Bank Street College of Education includes the following projects under the general direction of Barbara Biber:

1. The selection of psychologically qualified teachers:

a. One group of projects, supported in part by the United States Public Health Service, utilizes a projective technique designed to measure teaching-related personality characteristics. The study concerns the interrelationships among three sets of data: (1) responses to the projective tests obtained at time of admission to teacher-training; (2) evaluation of the students during the training year, and (3) intensive observations of on-the-job teaching performance.

b. A second study, supported by the Citizens' Committee on Children of New York City and conducted with the cooperation of the New York City Board of Education, involves the relations between teachers' responses to a number of techniques measuring personality attributes and their subsequent performance as teachers on-the-job.

2. A preliminary, descriptive study of young (nursery age) children's responses to stories of varying emotional content.

3. A descriptive analysis of procedures used at the Bank Street College for the job-related guidance of graduate teacher-trainees.

4. An additional project, still in the planning stages, will be concerned with the effects of differing kinds of school experience on children in the middle years, 9-11. This study will involve an assessment of: the nature of contrasting school atmospheres, the intellectual and emotional functioning of the children, the attitudes and ideology of parents and teachers with respect to methods and goals of child-rearing and education.

The following projects are being sponsored by Special Devices Center, Long Island, New York:

1. The Relationship of Primary Mental Abilities to Learning from Instructional Films (Radlow)
2. An Investigation of the Selectivity of Perception of Mental Health Films by Normal and Deviate Groups with Respect to Learning, Identification and Misperception (E. Stein)
3. Learning and Retention of English-Russian Vocabulary under Different Conditions of Motion Picture Presentation (Kale)
4. Sex, Age and Iconicity as Factors Influencing Projection onto Motion Picture Protagonists (McIntyre)
5. The Development of a Film Evaluation Form for Use by a Trained Rater Panel (Stover)
6. The Application of Sound Motion Pictures as a Means of Changing Attitudes toward Foods (Scollon, McIntyre and Guthrie)
7. The Evaluation of Three Army Training Films (Greenhill, Stein and VanderMeer)
8. The Relative Effectiveness of Films and Kinescopes (Hurst)
9. The Relative Effectiveness in Charts, Mock-ups and Cutaways in Teaching Nomenclature and Function of the 40mm Anti-Aircraft Gun and Mark 13 Type Torpedo (Torkelson)
10. The Production and Testing of Experimental Film Loops on the M-1 Rifle (McCoy)
11. A Methodological Study of the Application of Motion Pictures to Billet Analysis (Instructional Film Research Program Staff).

The following project is sponsored by the Adjutant General's Office:

12. The Development of a Motion Picture Test of Tank Mechanic Proficiency (Instructional Film Research Program Staff).

# Convention Calendar

**American Psychological Association:** September 4-9, 1953; Cleveland, Ohio  
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1333 Sixteenth Street N. W.  
Washington 6, D. C.

**Florida Psychological Association:** April 23-25, 1953; Miami, Florida  
*For information write to:*  
Dr. Theron Alexander  
Department of Psychology  
Florida State University  
Tallahassee, Florida

**Eastern Psychological Association:** April 24-25, 1953; Boston, Massachusetts  
*For information write to:*  
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Department of Psychology  
University of Delaware  
Newark, Delaware

**West Virginia Psychological Association:** April 24-25, 1953; Charleston, W. Va.  
*For information write to:*  
Dr. Frieda K. Merry,  
Morris Harvey College  
Charleston, West Virginia

**American Association for Cleft Palate Rehabilitation:** April 27-28, 1953; Atlanta, Georgia  
*For information write to:*  
Dr. Willard T. Hunnicutt  
302 Medical Arts Building  
Atlanta, Georgia

**Midwestern Psychological Association:** May 1-2, 1953; Chicago, Illinois  
*For information write to:*  
Dr. Lee J. Cronbach  
Bureau of Research and Service  
University of Illinois  
1007½ South Wright Street  
Champaign, Illinois

**American Society of Group Psychotherapy and Psychodrama:** May 4-5, 1953; Los Angeles, California  
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Laboratory of Social Relations  
Harvard University  
Cambridge 38, Massachusetts

**American Psychiatric Association:** May 4-9, 1953; Los Angeles, California  
*For information write to:*  
Dr. Cullen Ward Irish  
1930 Wilshire Boulevard  
Los Angeles 5, California

**Acoustical Society of America:** May 7-9, 1953; Philadelphia, Pennsylvania  
*For information write to:*  
Dr. Harry F. Olson  
RCA Laboratories  
Princeton, New Jersey

**California State Psychological Association:** May 9, 1953; Los Angeles, California  
*For information write to:*  
Dr. James F. T. Bugental  
Department of Psychology  
University of California  
Los Angeles 24, California

**Pennsylvania Psychological Association:** May 9, 1953; Harrisburg, Pennsylvania  
*For information write to:*  
Dr. William U. Snyder  
Department of Psychology  
Pennsylvania State College  
State College, Pennsylvania

**AERO Medical Association:** May 11-13, 1953; Los Angeles, California  
*For information write to:*  
Dr. T. H. Sutherland, Secretary-Treasurer  
P. O. Box 26  
Marion, Ohio

**American Association on Mental Deficiency:** May 12-16, 1953; Los Angeles, California  
*For information write to:*  
Dr. Neil A. Dayton  
P. O. Box 96  
Willimantic, Connecticut

**Canadian Psychological Association:** May 28-30, 1953; Kingston, Ontario, Canada  
*For information write to:*  
Dr. G. A. Ferguson, Secretary-Treasurer  
3544 Peel Street  
Montreal, P. Q., Canada

**American Psychopathological Association:** June 5-6, 1953; New York City  
*For information write to:*  
Dr. Donald M. Hamilton  
121 Westchester Avenue  
White Plains, New York

**American Neurological Association:** June 15-17, 1953; Atlantic City, New Jersey  
*For information write to:*  
Dr. H. Houston Merritt  
710 West 168th Street  
New York 32, New York

**Western Psychological Association:** June 18-20, 1953; Seattle, Washington  
*For information write to:*  
Dr. Richard Kilby  
Department of Psychology  
San Jose State College  
San Jose, California

**Association for Physical and Mental Rehabilitation:** July 20-24, 1953; Washington, D. C.  
*For information write to:*  
Dr. John Eisele Davis  
3423 S. Utah Street  
Arlington, Virginia

**Association Internationale de Psychotechnique:** July 27-August 1, 1953; Paris  
*For information write to:*  
Pr. R. Bonnardel  
41, rue Gay-Lussac  
Paris 5<sup>e</sup>, France

**International Sociological Association:** July 27-August 4, 1953; Liege  
*For information write to:*  
Mr. Erik Rinde  
Grev Wedels pl. 4  
Oslo, Norway

**Gerontological Society:** August 25-27, 1953; San Francisco, California  
*For information write to:*  
Dr. Harold E. Jones  
Institute for Child Welfare  
University of California  
Berkeley 4, California

**Society for the Study of Social Problems:** August 29-September 1, 1953; Berkeley, California  
*For information write to:*  
Professor Byron L. Fox, Secretary  
Society for the Study of Social Problems  
Syracuse University  
Syracuse, New York

**American Sociological Society:** August 30-September 1, 1953; Berkeley, California  
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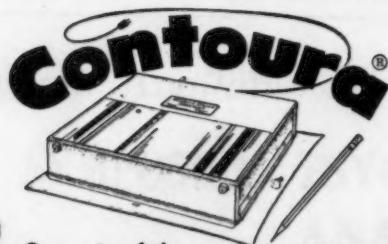


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